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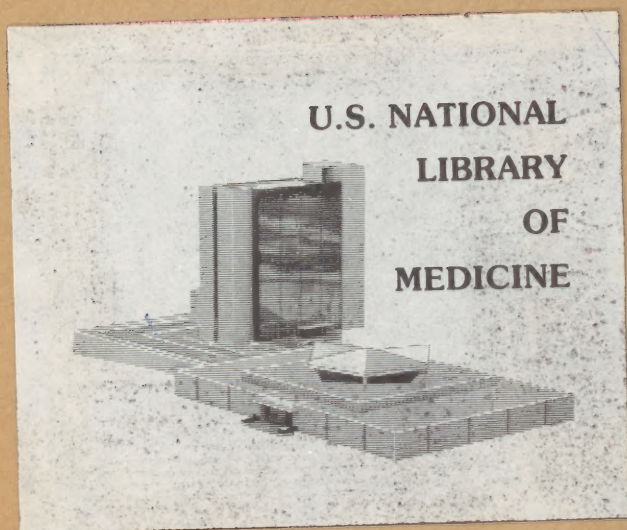
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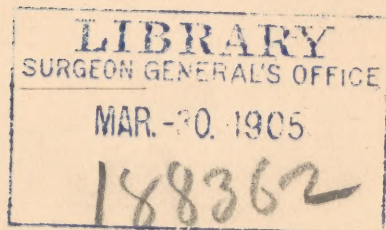
May, 1892.

On the Perception of Small Differences

WITH SPECIAL REFERENCE TO THE EXTENT, FORCE
AND TIME OF MOVEMENT.

BY

THE EDITORS.



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ON THE Perception of Small Differences

WITH SPECIAL REFERENCE TO THE EXTENT,
FORCE AND TIME OF MOVEMENT.

INTRODUCTION.

SECTION I. *The Least Difference which can be Perceived.*—Instruments discover differences which the unaided sense cannot detect. Sensations may appear alike to common observation, while the stimuli differ by amounts which may be measured with the help of rules, clocks, balances, thermometers and other instruments. We thus find a psychological problem which admits of scientific study. How great a difference must there be between two magnitudes in order that it may be perceived, and how does this least noticeable difference vary with varying circumstances? The relation between the intensity of a stimulus and the least noticeable difference is the only problem which has hitherto received much attention. But the intensity of the stimulus is only one of a number of factors affecting the error of observation. The time and space relations of the stimulus are not less essential than its intensity; and the variations of different observers, and of the same observer in different conditions, deserve study. The determination of the relation of one magnitude to another makes of psychology an exact science, whereas the study of personal differences may find practical applications in medicine and education.

We shall first review the methods which have been used in studying the perception of small differences, as these throw light on the nature of the mental process.

SEC. 2. *Method of the Just Noticeable Difference.*—If it be wished to determine the least difference between two stimuli which can be noticed, the simplest and most natural way would seem to be to make stimuli so different that the difference can just be noticed. This was indeed the method used by Weber¹ and by Fechner² in their first experiments on discrimination. It has been elaborated and praised by Wundt³ and others, but has not escaped criticism.⁴ Our own experiments have led us to the discovery of serious practical and theoretical objections to this method. These cannot be fully discussed in this paragraph. It is, however, evident that if it be left to the observer to say when he can just notice a difference, different observers may attach different meanings to the words. Lotze⁵ and others adopt the curious supposition that stimuli seem exactly alike so long as the difference is less than a certain amount, whereas, when the difference is made greater than this amount, it becomes

¹ Ernst Heinrich Weber, *Annotationes de Pulsu, Resorptione, Auditu et Tactu*, Lipsiæ, 1834; *Der Tastsinn und das Gemeingefühl*, in *Wagner's Handwörterbuch der Physiologie*, III. 2, Braunschweig, 1846; *Annotationes Anatomicæ et Physiologicæ*, Lipsiæ, 1851.

² Gustav Theodor Fechner, *Zend-Avesta*, Leipzig, 1851; *Elemente der Psychophysik*, Leipzig, 1860; *In Sachen der Psychophysik*, Leipzig, 1877; *Revision der Hauptpunkte der Psychophysik*, Leipzig, 1882.

³ Wilhelm Wundt, *Grundzüge der Physiologischen Psychologie*, Dritte umg. Aufl., Leipzig, 1887; *Preceding editions* 1880 and 1874; *Das Weber'sche Gesetz und die Methode der Minimaländerungen*, Lipsiæ [1882]; *Ueber die Methode der Minimaländerungen*, *Philosophische Studien*, Leipzig, 1883, I. 556.

⁴ Georg Elias Müller, *Zur Grundlegung der Psychophysik*, Berlin, 1879. Joseph Jastrow, *a Critique of Psychophysical Methods*, *Am. Journal of Psychology*, 1888, I. 271. Both of these writers, however, think that the method can be used in a correct manner—namely, by gradually increasing or decreasing a stimulus until the observer notice the direction of the change. This may, in some cases, be an improvement in technique, but does not seem to us to alter the fundamental assumptions of the method.

⁵ Hermann Lotze, *Metaphysik*, Leipzig, 1879 (English translation, Oxford, 1884), § 259, "Ein Intervall bleibt, durch welches hindurch der Reiz sich erfolglos verstärkt, um erst mit dem erreichten Endwerthe einen merklichen Unterschied der Empfindung hervorzubringen."

suddenly apparent. This is by no means the case. The clearness with which a difference is distinguished varies gradually from complete doubt to complete certainty. The variation is continuous, and no point can be taken and called the "just noticeable difference," and kept constant for different observers, or even for the same observer at different times. If complete certainty be taken as the standard, a difference in the stimuli will be required much greater than that which can ordinarily be distinguished, and the standard will be found to differ greatly with different observers, measuring, if anything, rather their character than their fineness of sensation. The clearness of discrimination is a continuous function of the amount of difference between the stimuli, but even with a fixed amount of difference it does not remain constant. It depends on many variables, so that a difference which may be distinctly perceived at one time will be indistinguishable at another. It has always been assumed that these variations may be eliminated by using the average of a large number of observations, but as they affect each single observation, they make it extremely difficult for a conscientious observer to come to any decision.

The only means by which scientific results can be obtained with the method of the just noticeable difference would seem to be to keep the observer in ignorance of the real relations of the stimuli, and see to it that when he said he could just notice a difference, the difference was of the sort he thought it to be. If in a long series of trials the observer make no mistake with a given difference, we may, indeed, assume that he can really distinguish this difference. But we should also conclude that he might notice a smaller difference, and have no basis for comparing different observers, or the same observer under different circumstances. If we make the difference smaller until the observer be sometimes wrong in his judgment, the method is

reduced to that of right and wrong cases which we have next to consider.

SEC. 3. *Method of Right and Wrong Cases.*¹—If two stimuli be taken so nearly alike that an observer cannot always distinguish the difference between them with clearness and certainty, and a series of trials be made, he will sometimes notice the difference distinctly, at other times vaguely or not at all, at still other times a difference will apparently be observed contrary to the real relation of the stimuli. Owing to the complex physical, physiological and psychological antecedents of perception, the same stimulus is not always accompanied by the same sensation. There is a normal error of observation; it has been treated by mathematicians and students of the physical sciences with a view to elimination, but it can be studied with advantage by the psychologist. Experience has led to the conclusion that small errors of observation are more common than large ones, and that an error is as likely to be positive as negative. The theory of probability further assumes that an error of observation is the algebraic sum of a very large number of small errors due to independent causes. Based on these assumptions, the probability of an error of any size has been determined by mathematical analysis, giving the well-known probability curve and its equation:²

$$y = ce^{-h^2 x^2}$$

What happens when an observer tries to distinguish a small difference may be illustrated by a diagrammatic figure (Fig. 1). Let the two magnitudes be proportional to the lines xo , XO ,

¹ This method seems to have been first used by Hegelmayer working under Vierordt's direction (Vierordt's Archiv, XI. 844). Fechner (*op. cit.*), however, deserves the chief credit for its elaboration. See, also, Müller (*op. cit.*).

² This paper is not written by mathematicians, nor for mathematicians. We give the commonly accepted formulæ, but our results may be understood by those who are not familiar with their significance. The theory of errors of observation is treated without mathematics by Jevons, Venn and Galton, and with the aid of comparatively simple mathematics by Airy and Merriman.

their difference would then be PO . Now, when the observer perceives the first stimulus x_0 , the error of observation will cause him to perceive it as sometimes smaller, sometimes larger than x_0 . The distribution of his errors is represented by the curve vyn . That is, the probability of any error, say po , is proportional to the corresponding ordinate pq , or the number of errors in one direction not greater than po is proportional to the por-

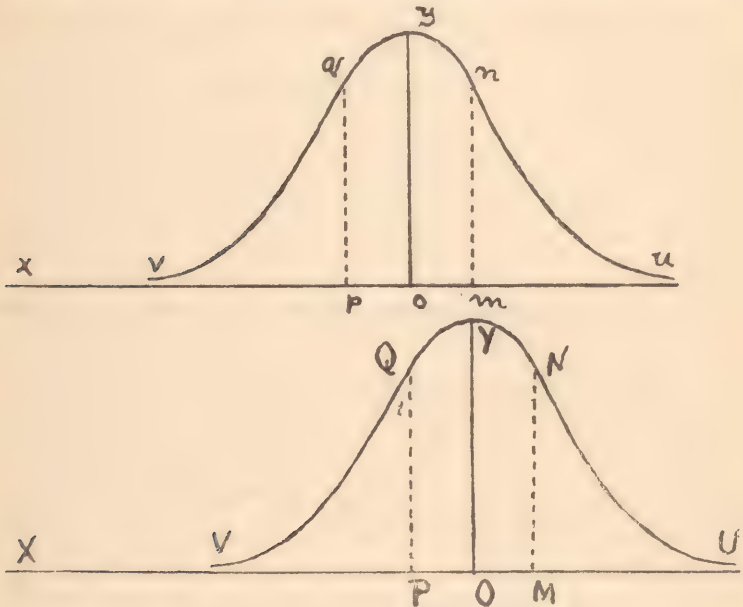


FIG. 1.—THE DISTRIBUTION OF ERRORS OF PERCEPTION.

tion of the area of the curve above this abscissa, $po yq$. The shape of the curve depends on h and c in the equation, and these depend on the average precision of observation. Thus if the curve vyn represent the precision of one observer, the precision of a better observer would be represented by another bell-shaped curve taller and narrower than vyn .

This curve may be drawn if any one abscissa and its ordinate be known. An abscissa of such a size that half the errors are

smaller and half larger (in the figure this would be po) may conveniently be used as a measure of the accuracy of discrimination. This abscissa or error is called the probable error.¹

The error of observation cannot, however, be directly determined. After the stimulus xo has been perceived, a second (say greater) stimulus, XO , is presented to the observer. Supposing the perception of this stimulus to be subject to the same error of observation as the first,² the distribution of errors is represented by the curve VYU . Now, a consideration of these two curves will show that the first stimulus might be perceived as xv , and the second as XU , in which case the difference would be perceived with distinctness and certainty. But more frequently the first stimulus would be perceived as larger than xv , say as xp , while the second stimulus might be perceived as XM ; in this case the observer might distinguish the difference, but it would seem smaller than before, and would be noticed with less clearness and confidence. It is further possible that the two stimuli might be perceived respectively as xo and XP , in which case they would seem alike. Lastly, the first stimulus might be perceived as xu , while the second stimulus might be perceived as XV , in which case the objectively greater stimulus would appear the less. If the observer could define the amount of difference which he perceives in a series of trials, his curve of precision could be immediately drawn, but he can only do this vaguely.³ He may, however, be required to say which of the

¹ The mean error, the error of mean square, and the modulus are all used to measure the error of observation. These quantities are related theoretically. If the probable error be 1.00, the mean error is 1.18, the error of mean square 1.48 and the modulus 2.10.

² It would usually be different, but to an inconsiderable degree. (See Müller's criticism, *op. cit.*, p. 21 *seq.*).

³ We indeed required the observer to give the degree of confidence which he felt in his judgment, and noted it by the letters a, b, c, d —a plan proposed by Peirce and Jastrow (C. S. Peirce and J. Jastrow, On Small Differences of Sensation, National Academy of Sciences, III, [1884]). The great variation in the use of these letters (say " a " practically sure) by different observers shows how little reliance can be placed on their judgment of the just noticeable difference, Method I.

two stimuli seems the greater, and the number of times he is right and the number of times he is wrong may be recorded.¹ If, with the same difference, two observers be right in their decisions the same percentage of times, we conclude that their accuracy of discrimination is the same. If one observer be right a larger percentage of times than another, the difference in the stimuli can be decreased for the better observer, until he be right the same percentage of times, and we regard the accuracy of discrimination of the two observers as proportional to the difference which they can discriminate equally well. In the same manner the accuracy of the same observer under varying conditions (for example, when both magnitudes are greater, or when the time between their presentation is increased) may be determined.

It would be tedious to experiment until a difference were found for which the discrimination of different observers should be alike, nor is this necessary. It is evident that the percentage of right cases will increase as the difference is taken greater, and the relation between the two can be determined by mathematical analysis. It follows the formula developed by Gauss for determining the relative area of the probability curve above any abscissa. Adapted to the percentage of right cases it is

$$\frac{r}{n} = \frac{1}{2} + \frac{1}{\sqrt{\pi}} \int_0^{t = \frac{\Delta}{P.E.}} e^{-t^2} dt$$

in which *P.E.* is the probable error and Δ the amount of differ-

¹ As we have seen, the stimuli might appear alike, and most experimenters have recorded right, wrong and doubtful cases. Strictly speaking, however, the two stimuli would never appear exactly alike (except once in an indefinitely large number of trials), and it is theoretically better and practically more convenient to require the observer to come to some decision. "Doubtful cases" are analogous to the just noticeable difference, depending on the judgment (or caprice) of the observer. The assumption that there is no difference in sensation when the observer is doubtful as to its nature has led Fechner, Müller, Wundt and others into many difficulties.

ence.¹ The values of this integral given in works on probability are the converse of what is needed for the present purpose. We, therefore, give a table in which the entries and arguments have been reversed. This table gives the values of $\frac{\Delta}{P.E.}$ for every percentage of right cases above fifty.²

TABLE I.

TABLE FOR DETERMINING THE PROBABLE ERROR FROM THE PERCENTAGE OF RIGHT CASES AND AMOUNT OF DIFFERENCE.

%r	$\frac{\Delta}{P.E.}$	%r	$\frac{\Delta}{P.E.}$	%r	$\frac{\Delta}{P.E.}$	%r	$\frac{\Delta}{P.E.}$	%r	$\frac{\Delta}{P.E.}$
50	.00	60	.38	70	.78	80	1.25	90	1.90
51	.04	61	.41	71	.82	81	1.30	91	1.99
52	.07	62	.45	72	.86	82	1.36	92	2.08
53	.11	63	.49	73	.91	83	1.41	93	2.19
54	.15	64	.53	74	.95	84	1.47	94	2.31
55	.19	65	.57	75	1.00	85	1.54	95	2.44
56	.22	66	.61	76	1.05	86	1.60	96	2.60
57	.26	67	.65	77	1.10	87	1.67	97	2.79
58	.30	68	.69	78	1.14	88	1.74	98	3.05
59	.34	69	.74	79	1.20	89	1.82	99	3.45

The values of $\frac{\Delta}{P.E.}$ in this table are inversely proportional to the probable error of an observer, and measure directly his accuracy of discrimination. Thus if two weights be used weighing 100 and 108 gm., and one observer can correctly distinguish the difference ninety-one per cent. of the time, while another can distinguish it only seventy-five per cent. of the time, the accuracy of the former is about twice as great as that of the latter, or the observer who could distinguish a difference of 8 gm. ninety-

¹ We had in view the testing of this formula by experiment; it had, however, been confirmed by Fechner, by Peirce and Jastrow and during the course of our experiments by Higier (*Experimentelle Prüfung der psychophysischen Methoden im Bereiche des Raumsinnes der Netzhaut*. Dorpat, 1890. Also *Philos. Stud.*, VII. 232, 1891).

² In case the percentage of right cases be less than fifty, which can only occur when there is a constant error larger than Δ or when the number of experiments is limited, use the percentage of wrong cases, and take $\frac{\Delta}{P.E.}$ negative.

one per cent. of the time could distinguish a difference of 4 gm. seventy-five per cent. of the time. Thus whatever the difference between the stimuli and whatever the percentage of right cases, the difference with which an observer would be right seventy-five per cent. of the time can be discovered.

The equation given above can, of course, only be solved with the aid of the integral calculus, but its meaning may be understood by regarding Fig. 1. Thus if the difference between the stimuli be PO , the number of times that XO will seem larger than xo or XP will be to the number of times that it will seem smaller, as the area $PUNQ$ is to the area PVQ , but in this case the area $PUNQ$ is to the area PVQ as 3:1; consequently, when the difference (Δ) is equal to the probable error ($P.E.$), the observer will be right in his decision seventy-five per cent. of the time.¹

We trust the considerations brought forward in this section will throw light on the real nature of the just noticeable difference. It seems to us that it is a difference so large that the error of observation will not often cause the observer to err as to its nature. We learn by experience what difference we can usually distinguish correctly, and regard this as the just noticeable difference. But what point shall be called the just noticeable difference depends on the character and experience of the observer. Some people are sure they are right when they have but little to guide them, whereas others are slow in coming to a decision. If this view of the just noticeable difference be cor-

¹ While the probable error seems the best standard for comparison, it is better in making experiments to choose a difference with which the observer will be right about eighty-four per cent. of the time, the error of mean square. This is an abscissa about half again as large as PO (Fig. 1), where it will be seen there is an inflection point in the curve, and it approaches the horizontal axis most rapidly. Here an alteration in the error makes the greatest difference in the size of the ordinate, and a reliable result is reached with fewest experiments. Near V and near V' , on the other hand, the curve is nearly parallel to the horizontal axis, and very large (near 100) or very small (near 50) percentages of right cases are the most unfavorable for calculating the probable error.

rect, it cannot be regarded as a fixed unit fit for measuring the intensity of sensation, and preceding theories on this subject must be radically altered.

SEC. 4. *Method of Average Error*.—If a stimulus be presented to an observer, and he be required to make a second stimulus as nearly as possible equal to it, the error of observation which we have been considering will lead him to make it sometimes greater, sometimes less. If a series of trials be made and the average error taken, the observer's accuracy of discrimination is directly measured. As (apart from constant errors) the real errors are known, the average error directly obtained is proportional to the probable error (1.000 : 0.845), and results obtained by this method may be readily compared with those obtained by the method of right and wrong cases. By the latter method, however, we only determine the number of errors in one direction greater than a given size, and many experiments must be made before we can proceed to calculate the probable error, whereas by the method of average error the amount and direction of error are in each case measured, and a reliable result is obtained more directly and quickly. A single experiment by the method of average error indicates the observer's accuracy of discrimination, and half-a-dozen give a result sufficiently reliable for clinical and anthropometric purposes. This method is therefore to be preferred whenever it can be used. If, however, the observer be required to make a stimulus, such as a weight or light, apparently equal to another, he can only do so with repeated trials, and it is difficult to keep the conditions constant. In such a case it is better to use a fixed difference and the method of right and wrong cases. With movements, on the other hand, the method of average error can be used to great advantage. The observer is required to make one movement and then another movement as nearly as possible equal to it, the error in extent,

force or time being measured by suitable apparatus. In this case, however, the error is complex, being partly an error of perception, and partly an error in adjusting the second movement. We do not see how the error of adjustment can be eliminated otherwise than by using the method of right and wrong cases.

The error of an observer is apt to consist of two components—one a variable error dependent on the normal variation in perception, which we have been considering—the other a constant error due to overestimating or underestimating the second stimulus as compared with the first. With the method of average error the constant error is directly determined by the experiments, and it may be subtracted algebraically from each error, and thus the variable error obtained.¹ The precision of observation has usually been measured by the variable error, but the constant error also measures an important error in perception.

SEC. 5. *Method of Estimated Amount of Difference.*—When experiments are made with stimuli of different degrees of intensity, a stronger and a weaker stimulus may be presented to the observer, and he may be required to fix on a third stimulus which appears to him midway between the two. This has been called the method of mean gradation.² An observer may further be required to double³ a given stimulus, also to halve it, etc. We regard this method as distinct from the three preceding methods, which rest on the error of observation.⁴ It is true, the error of

¹ The theoretical relation of the variable to the constant error is determined by values of P , E , and Δ in the probability integral. See, below, the method of eliminating and determining the constant error, when the method of right and wrong cases is used.

² This method has long been used by astronomers in estimating the apparent magnitudes of the stars. It is said to have been first used for psychological purposes by Plateau (Bull. d l'Acad. de Belgique, XXXIII. 376, 1872).

Apparently first done by Merkel, Philos. Stud., IV. 541; V. 245, 499, 1888-9.

⁴ The method of just noticeable difference has been used in such a form that the differences should appear not just noticeable, but equal. In this case the method comes under the present head, and is subject to the same criticism.

observation is measured in this method, but, in addition, the observer attempts to estimate definite quantitative differences in sensation. If an observer can, in fact, estimate quantitative amounts of difference in sensation, apart from association with known quantitative differences in the stimuli, a relation between mental and physical intensity can be determined. The writers, however, agree in finding that they cannot estimate such quantitative differences in sensation in a satisfactory manner. We can indeed say when one weight seems approximately double another, but this is doubtless because we have often lifted first one volume, and then two, and the like. But we cannot say when one sound seems twice as loud, or one day twice as hot as another. We have made experiments to see how nearly different observers would agree in adjusting one shade of light midway between two others, and have found hesitation in coming to a decision and great divergence of opinion. Most men will think that a just king is happier than a tyrant, but few will agree with Plato in considering him 729 times as happy.

An important distinction in the senses should be kept in mind. In estimating the quantitative relations of weights, for example, we should expect an approach to the true relations. We know when one weight is twice as heavy as another, because two objects of the same sort are twice as heavy as one, and from frequent use of the balance. We are also interested in the objective relations of weights. Two pounds of sugar cost twice as much as one, and go twice as far. Sounds and lights, however, are rarely doubled, and can be measured only with difficulty, if at all. We, therefore, have no association with the real magnitudes of the stimuli. On the other hand, we see the same object, sometimes in a bright light, sometimes in a dim light; we hear the same noise, sometimes near by, sometimes at a distance. We may, therefore, come to regard relative differences as equal differences.

SEC. 6. *The Laws of Weber and of Fechner.*—The methods we have been considering have been used chiefly in studying the relation between the least noticeable difference and the intensity of the stimulus. Weber¹ observed that the least difference between two stimuli which could be noticed was not the same for stimuli of different magnitudes, but a proportional part of the stimulus. When an observer could just distinguish the difference between 1 and 1.05 kg., he could not distinguish any difference between 4 and 4.05 kg., but could only distinguish the difference when it was about four times as great—*i.e.*, about .2 kg.

Since Weber published this result, an amount of experiment and theorizing has been devoted to this subject, which has probably never been surpassed in the history of any science. It is commonly claimed that the experiments confirm Weber's generalization. At all events, the difference which can just be noticed is usually found to become greater as the stimulus is taken greater. Weber's law can be expressed by the equation

$$N = C \frac{\Delta S}{S}$$

in which N is the least difference which can be noticed, and ΔS the increase in the stimulus S which causes this difference. No objection can be made to this statement of Weber's law so long as the meaning of N be left an open question. We ourselves consider it a physical quantity, dependent on the error of observation, and this indeed, if Weber's law hold,

¹ *Op. cit.* As long ago as 1730 Daniel Bernoulli (cf. Todhunter, History of the Theory of Probability, Cambridge and London, 1865) maintained that the value of money is relative, so that (to use the terms of Laplace) the *fortune morale* is proportional to the logarithm of the *fortune physique*. The general principle is expressed by Hobbes and Spinoza, and is implicit very early, as in the institution of Mosaic tithes. Bouguer (Traite d'optique, etc., Paris, 1760) seems to have been the first to observe that the just noticeable difference (between lights) is proportional to the magnitude of the stimulus. Lambert (Neues Organon, Leipsic, 1764, II. 268-9) seems to have made the same observation independently, as his least noticeable difference ($\frac{1}{30}$) differs from that of Bouguer ($\frac{1}{40}$).

would increase in direct proportion to the stimulus. Fechner, followed by many others, regards N as an equal increase in sensation, always the same whatever value may be given to S . If N be such a unit fit for measuring the intensity of sensation, it follows directly from Weber's law that the sensation increases in arithmetic ratio as the stimulus increases in geometric ratio. Fechner further assumes that the difference between no sensation and the least sensation which can be noticed is a mental quantity equal to the other N 's, whence follows the equation :

$$E = C \log. S$$

that is, the sensation is equal to the logarithm of the stimulus multiplied by a constant. This equation, which leads to negative sensations and other difficulties, has been discussed at great length by many writers.¹

SEC. 7. *Interpretations of Weber's Law.*—According to Wundt and other writers, there are three possible interpretations of Weber's law. These are respectively psychophysical, physiological and psychological. The first is adopted by Fechner, who holds that his development of Weber's law expresses an ultimate relation between mind and matter. Such a func-

¹ Some of the more important works not hitherto mentioned in this paper are as follows:

H. Aubert, *Physiologie der Netzhaut*, Breslau 1865.

H. Helmholtz, *Handbuch der Physiologischen Optik*, Leipzig 1867. Second edition in course of publication.

J. J. Müller, *Sitz. d. Sächs. Ges.*, Leipzig 1870.

J. Delbœuf, *Étude psychophysique*, Bruxelles, 1873, and later publications.

F. Brentano, *Psychologie, etc.*, Leipzig 1874.

E. Hering, *Ueber Fechner's psychophysisches Gesetz*, *Sitz. d. Wiener Acad.* 1875.

P. Langer, *Die Grundlagen der Psychophysik*, Jena 1876.

F. A. Müller, *Eine Untersuchung etc.*, Marburg 1882.

J. v. Kries, *Ueber die Messung etc.*, *Viertelj. f. wissensch. Philos.*, VI. Leipzig 1882.

P. Tannery, *Revue Philos.*, Paris 1884.

A. Elsas, *Ueber die Psychophysik*, Marburg 1886.

A. Köhler, *Ueber die hauptsächlichsten Versuche etc.*, *Philos. Stud.*, Leipzig 1886.

A. Grotenfelt, *Das Weber'sche Gesetz etc.*, Helsingfors 1888.

H. Münsterberg, *Beiträge etc.*, Freiburg i. B. 1889.

tional relation is, of course, only conceivable on the assumption that the sensation is measurable. If this be the case, however, a direct proportion would seem far more natural, and should only be rejected under stress of facts which cannot otherwise be explained. The physiological interpretation of Weber's law is based on the supposition that the sensation is, indeed, directly proportional to the brain changes immediately correlated with it, but that these increase as the logarithm of the physical stimulus. It has been argued that this may be due to resistance,¹ radiation² or fatigue³ in the nervous system. Owing to our complete ignorance of the minute physiology of the nervous system, such hypotheses may be made more easily than proved or disproved. The inertia of the nervous system may account for a very weak stimulus not being perceived, and its exhaustion or destruction for a very strong stimulus failing to call forth a corresponding sensation, but so long as there is no proof it must be regarded as unlikely that brain changes should vary as the logarithm of the stimulus. A psychological explanation of Weber's law is favored by Wundt, who argues⁴ that it is a case of the "relativity of feeling." This principle may account for certain effects of contrast, etc., but scarcely seems relevant to the supposed relation between intensity of sensation and intensity of stimulus. Indeed, Wundt seems to think that the physiological explanation may be combined with his.

SEC. 8. *The Relation between the Error of Observation and the Magnitude of the Stimulus.*—As may be gathered from what has preceded, the writers cannot accept any of these explanations of Weber's law. All the experiments made by the first three

¹ J. Bernstein, *Untersuchungen über den Erregungsvorgang im Nerven- und Muskel-Systeme*, Heidelberg 1871.

² James Ward, *An Attempt to interpret Fechner's Law*, Mind I. 464, London 1876.

³ Herm. Ebbinghaus, *Ueber den Grund der Abweichungen von dem Weber'schen Gesetz bei Lichtempfindungen*, Pflüger's Archiv, XLV. 113, 1889.

⁴ Phys. Psy., I. 377 *seq.*

methods which we have described seem to us to determine the error of observation under varying circumstances, and not to measure at all the quantity of sensation. The fourth method, it is true, seeks to estimate quantitative relations in sensation, but we find that this cannot be done apart from association with known quantitative relations of the stimuli. Our own experiments by this method, to be described below, show that in such cases we tend to estimate the intensity of sensation as directly proportional to the intensity of the stimulus; consequently, in so far as any deduction concerning quantitative relations in sensation can be made from such estimation, the sensation increases as the stimulus and not as its logarithm. The experiments which we have made by the first three methods (and incidentally by the fourth) show that the error of observation usually increases as the stimulus is taken greater, but more slowly. We therefore believe that Weber's law does not hold for the perception of movement. We believe, however, that the error of observation tends to be related to the magnitude of the stimulus in a simple manner; namely, that the error of observation (and the so-called least noticeable difference) is proportional to the square root of the stimulus. We believe, further, that we can explain this relation in a satisfactory manner. Suppose time to be taken as the physical magnitude. If one second be estimated, a certain error of observation will occur. If the experiment be repeated, an error on the average of the same size will be made. Now, if the two seconds be estimated continuously, each second composing the interval may be regarded as subject to its own error. If these errors were always in the same direction, the error in estimating two seconds would be twice as great as the error in estimating one second, and Weber's law would hold. In half the cases, however, the two errors would be in opposite direc-

tions and would partly counterbalance each other, and the average error in estimating two seconds would, consequently, be smaller than twice the average error for a single second. It would, indeed, according to the theory of probability (confirmed by our own experiments), be the average error for one second multiplied by the square root of two. Or, generally, the algebraic sum of a number of errors is the average error multiplied by the square root of the number. There seems to be reason for maintaining that this deduction should hold for the estimation of any physical magnitude composed of equal units, although this view has not, we think, been hitherto proposed.

As the result of our own experiments we find that the error of observation varies more nearly as the square root of the magnitude than as the magnitude. An exact correspondence with such a law can indeed only be looked for under the simple conditions assumed by the theory of probability. In actual perception the fractions of the magnitude, physically equal, would seldom or never be subject to exactly the same errors of observation. Every general law is subject to secondary laws, and the greater our knowledge the more do these secondary laws increase. The planets do not, in fact, move in ellipses, but by very complex paths; the volume of a gas does not, in fact, vary inversely as the pressure, but in a very complex manner. It is usually a token of our ignorance rather than of our knowledge when the complex phenomena of nature seem to be related by a simple mathematical formula. The error of observation would, in fact, seldom or never increase as the square root of the magnitude, but the summation of errors would seem to account in an entirely satisfactory manner for the usual increase of the error of observation as the magnitude is taken larger. We, therefore, substitute for Weber's law the following: *The error of observation tends to increase as the square root of the magnitude,*

*the increase being subject to variation, whose amount and cause must be determined for each special case.*¹

SEC. 9. *The Perception of Bodily Movement.*—The larger part of the experiments described in this paper are concerned with the perception of small differences in movements of the body. We choose this class of sensations, partly because it offers peculiar advantages for studying the perception of small differences, and partly because more knowledge is needed concerning the perception of movement. Movements are convenient for such study, because the observer himself causes the stimulus which he perceives, while at the same time it can be measured more exactly than light or sound. The complexity of the perception of movement is so considerable that there is much difference of opinion as to its nature, and for its study exact experiments are needed.

Common observation does not tell us what nervous and muscular mechanism is involved in movement, nor what sensory apparatus is used in its perception. In this paper we shall lay stress on the fact that movements differ in their force, in their extent and in their time, and we shall try to show how accurately these different factors are discriminated, and how far they are interdependent. This analysis of movement seems to us natural, as it is in accordance with the magnitudes, energy, space and time, on which physical science is based. But we have still to inquire how we come to perceive the force, extent and time of our movements. The perception of movement was formerly identified with touch, and passive sensations from the skin undoubtedly give us knowledge concerning bodily movements. These

¹ One of the writers (G. S. F.) gives but a qualified assent to the subject-matter of this section and its application elsewhere in this paper, on the ground that mathematicians are not agreed as to the soundness of the theory upon which the law is based, and also on the ground that the errors in question may not be independent errors. He regards, however, the results obtained by the writers as sufficiently in accord with the law to justify them in holding it tentatively, and subject to criticism.

sensations may be of two sorts—one due to pressure at the place where force is applied, the other due to contraction, relaxation, etc., of the skin of the moving member and its connections. In addition to these sensations from the skin, sensory nerves ending in the muscles¹ and about the joints² have been discovered, and these doubtless play some part in the perception of movement.³ Then in all movements, and especially in such as are violent and powerful, many parts of the body are concerned. The whole position and balance of the body may be altered, and vital processes, respiration, circulation, etc., are affected. In addition to these sensations from the skin and inner body, important knowledge concerning the movement may be obtained from the other senses, particularly from sight.

It is well known that besides the numerous sensory impressions resulting from the movement, an immediate consciousness of the motor impulse calling forth the movement has been considered an essential factor in its perception. Wundt,⁴ Bain⁵ and others lay great stress on this “feeling of innervation,” holding it to be an important, if not the only ground for belief in matter and space. On the other hand, Bastian,⁶ Ferrier,⁷ James⁸ and others

¹ C. Sachs, *Archiv f. Anat. u. Phys.*, 1874, pp. 175, 491, 645. Sachs' microscopical results have, however, been disputed. See Mays, *Zeitsch. f. Biol.*, XX.

² Rauber, *Vater'sche Körper der Bänder und Periostnerven und ihre Beziehung zum sogenannten Muskelsinn*, München, 1865. See William James, *The Principles of Psychology*, New York, 1890, II. 189, *seq.*

³ Wundt (*Phys. Psy.*, I. 464) writes: “Die Annahme eines spezifischen Muskelsinnes wurde zuerst, wie es scheint, von Ch. Bell aufgestellt.” The distinction was probably known to Aristotle, and is plainly made by Scaliger (1557). See Hamilton's edition of Reid's Works, Edinburgh 1846, p. 867.

⁴ *Beiträge zur Theorie der Sinneswahrnehmung*, Leipzig 1862; *Vorlesungen über die Menschen- und Thierseele*, Leipzig 1863; *Phys. Psy.*

⁵ *Senses and Intellect*, 3d ed., 1868. The author's views were gradually elaborated in the preceding editions, 1855 and 1864.

⁶ On the Muscular Sense, *Brit. Med. Journ.*, 1869; *The Brain as an Organ of Mind*, appendix, 3d ed., London, 1882; *Brain*, 1887, with discussion.

⁷ *The Functions of the Brain*, 1st ed., London, 1876; 2d ed., 1886, Ch. X.

⁸ *The Feeling of Effort*, *Anniv. Mem. of the Boston Soc. of Nat. Hist.*, 1880, *Psy.*, Ch. XXVI.

argue that there is no consciousness accompanying the outgoing current. The extended discussion concerning the sense of innervation may be partly confused by misunderstanding. It is evident that we know what movement we purpose to make, but it seems equally evident that we cannot thus know what movement we carry out. We may will to lift a heavy box, and discover from the unexpected movement resulting that the box is empty. Our perception of the movement we have carried out may, however, be affected by our knowledge of the movement we have willed. The box which unexpectedly turns out to be empty seems to fly up of itself.

SEC. IO. *The Importance of Uniform Conditions.*—Before proceeding to describe our experiments, some mention of special methods should be made. One of the most important considerations is the need of keeping all the conditions constant, except the variable to be investigated. This is such an evident precaution in all exact experiments that it would seem needless to mention it, were it not that it has been neglected in many psychological investigations. The resulting confusion has been very great. If experiments are being made on the variation of the error of observation, with the magnitude of the stimulus, care must be taken to keep the time of stimulation, the interval between the stimuli, the place and it may be the area of stimulation, the condition of the observer, etc., constant. This has often been neglected, especially when the methods of just noticeable difference and of average error have been used. But the greatest confusion has been due to the previous knowledge of the observer. If he know the results of preceding experiments, he may seek to distinguish apparent differences in sensation, or he may seek to make his judgment objectively correct, and the difference in result will be great. If the observer know beforehand the real relations of the

stimuli in each case, he will find that this will affect the apparent relations of the sensations, and almost any result can be obtained. We regard, therefore, all experiments made by a single observer on himself as provisional. Lastly, many psychological researches have but little value because the author has rejected experiments and series which seemed wrong to him. Under such circumstances even the most conscientious observer may obtain results conforming better to his theory than to the truth.

SEC. II. *Concluding Remarks.*—The experiments published in this paper were mostly carried out in the psychological laboratory of the University of Pennsylvania during the years 1889, 1890 and 1891. The observers who took part in the experiments are designated by letters: *F.* and *C.*, the writers; *R.* and *J.*, respectively, their wives; *K.*, also a woman, a student of psychology and biology; *D.*, *N.* and *S.*, advanced students of psychology; *L.* and *S.*, advanced students of biology. The calculations were made by the observers. Most of the apparatus used was given to the University by Dr. Weir Mitchell, Mr. H. H. Houston and Mrs. Matthew Baird. Some part was obtained through an appropriation from the Bache Fund, and is the property of the National Academy of Sciences. The new apparatus was made under our direction by Clay & Torbensen, Camden, N. J. The drawings of apparatus were made by Miss Washington, University of Pennsylvania, and the curves were drawn by *F.*, *C.* and *J.*

PART I.

ON THE EXTENT OF MOVEMENT.

SEC. 12. *Introductory*.—Until quite recently but little experimental work had been done on the discrimination of the extent of movements. Wundt¹ gives the results of an investigation concerned with movements of the eyes, and considers them in harmony with Weber's law. Numerous experiments² have been made on the perception of small differences in the length of lines ; and as the eye in most cases has not been carefully fixed, the movement was doubtless an important factor in the judgment. But it cannot be separated from the influence of the size of the image. Several researches have been published comparing movements made with the right and left hands. Thus Hall and Hartwell,³ Loeb⁴ and Bloch⁵ have studied movements of the arm with a view to comparing those made with the right side of the body and those made with the left. Jastrow⁶ has investigated the perception of space by disparate senses, and in so doing has made use of various movements, but he is concerned rather with the relations which different senses, or different aspects of the same sense, hold to

¹ Phys. Psy, Bd. II. s. 116 ff.

² The most recent by Higier (*op. cit.*), who gives references to preceding investigations.

³ Bilateral Asymmetry of Function, Mind, Vol. IX. pp. 93-109.

⁴ J. Loeb, Untersuchungen etc., Pflüger's Archiv, 1887 and 1890.

⁵ A. M. Bloch, Experience etc., Rev. Scientifique, 1890.

⁶ The Perception of Space by Disparate Senses, Mind, Vol. XI. pp. 539-54.

each other than with the accuracy with which the extent of movements can be discriminated, in itself considered.¹

There are peculiar difficulties connected with the study of ocular movements, and it has seemed desirable to the writers to choose for investigation movements of another class. In judging by the eye of the distance between two points, or of the length of a line, active sensations of movement may contribute much to the result ; but the passive sensations arising from the extent, the position or the motion of the image on the retina, or in binocular vision, the difference in character of the retinal images, may contribute no unimportant factor. In free movements made by the arm this difficulty is obviated. They are, to be sure, complex, and in so far not wholly satisfactory ; but they are at least less unsatisfactory than ocular movements, and offer the special advantage that they enable us to compare directly the judgment of the extent of a movement with the judgment of its force and time.

Our experiments on the extent of movement were undertaken with a view to :

- (1) The investigation of psychophysics law in the sphere of bodily movements.
- (2) A comparison of the results obtained by the use of the four psychophysical methods.
- (3) The determination of the effects of practice.
- (4) The determination of the significance of the confidence of the experimentee in relation to objective correctness.
- (5) The investigation of constant errors.

¹ No less than four inaugural dissertations have recently been published bearing more or less directly on the subject. Those by Cremer and by Ostermann, Würzburg, and by Falk, Dorpat, we have not seen. *Ueber Bewegungsempfindungen*, by Edmund Burke Delabarre, Freiburg i. B., 1891, we have received since our paper has been written. This dissertation contains interesting experiments on movements of the arm, but the relation of the variable error to the magnitude of the movement is not considered.

SEC. 13. *Apparatus and Methods.*—The larger part of the experiments on the discrimination of the extent of movements were made with very simple apparatus. A scale, graduated to millimeters, was fastened along the edge of an ordinary table. At one end of the scale, to the left of the experimentee as he sat facing the table, was a fixed upright which served as point of departure for the movement. A movable upright, which could be placed at any desired point on the scale, determined the extent of the movement chosen as standard of comparison. To allow for the width of the finger used in making the movements, the two uprights were set on flat bases, with bevelled edges projecting in such a way that, when these edges were in contact at the 0 point of the scale, there was just room for the finger between the uprights above. A seconds pendulum, swinging above the table, gave the time for each movement, and the interval between the movements to be compared. The experimentee sat directly opposite the 10 cm. mark on the scale, with the fixed upright to his left at the 0 point. His body was about 20 cm. back from the edge of the table. The movement was made with the index finger of the right hand, the arm swinging freely and being used as a whole in the movement. The motion was from left to right, and when in motion the finger was from 2 to 6 cm. above the table. Each movement occupied, as nearly as possible, one second, and one second was allowed for recovering position between the two movements to be compared in each experiment. A screen prevented the experimentee from seeing the movements made. The recorder sat to the right of the experimentee and regulated the standard movement in each experiment by placing in position the movable upright. The experimentee then moved his finger over the space between the two uprights, and, having thus gained a standard, made a second movement to which the first served as guide. The point at

which the finger rested upon the scale at the end of this second movement was recorded, the finger-nail being cut to a point as a help to the recorder. The experimentee knew nothing of the results of the experiments until after the whole number was completed. The danger of his unconsciously allowing for constant errors, and vitiating the results, was thus avoided.

With this apparatus, and in the manner described, 4400 experiments were made, and all four of the so-called psychophysical methods were employed. The method of just noticeable difference was used in the attempt to mark off a distance just greater and one just less than 500 mm. The method of estimated amount of difference was used in the attempt to halve 500 mm., to double 300 mm., and to find the mean between 300 and 700 mm. The method of average error was used in the attempt to measure off successively 100, 300, 500 and 700 mm. Finally, the method of right and wrong cases was used in the attempt to decide whether the two uprights were nearer together or farther apart in the second of each pair of movements, their distances being as 500:510 mm., or as 510:500 mm.

We have here ten different kinds of experiments; or, since the experiments by the method of right and wrong cases may count double, the second movement in each experiment being half the time longer and half the time shorter than the first, we may say that we have eleven kinds. A complete series, including ten experiments of each kind, or in all 110 experiments, was made at each sitting. Sometimes one and sometimes two series were made in a day. The ten experiments of each kind (except those by the method of right and wrong cases) were made before passing to the next, and the eleven different kinds were arranged in the series in the order in which they are given above. Every second series was made in reversed order. Further details of these experiments will be given later, when each

class will be discussed by itself. All these experiments were made upon *F*, the recorder being *M*., who is not otherwise represented in this paper.

After these experiments were made, it occurred to the writers that there was a possible source of error in the method of getting the standard movement in most of the experiments. In all the experiments, the first movement was determined by two uprights, while the second movement, except in the experiments by the method of right and wrong cases, was made with the movable upright removed. It would seem to be quite possible

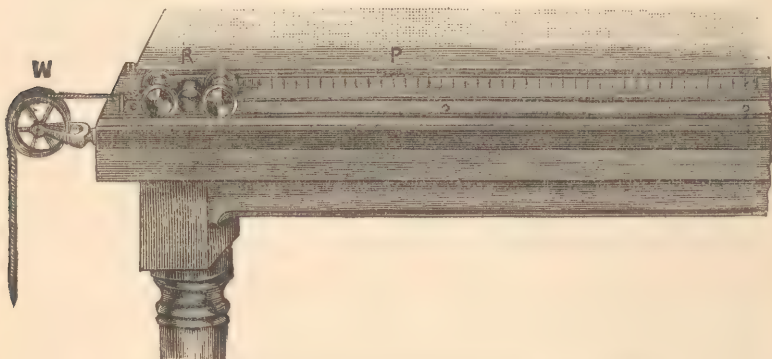


FIG. 2.—APPARATUS FOR MEASURING THE EXTENT OF MOVEMENT.

that the force expended in the touch against the upright limiting the first movement would have made the movement appreciably greater if the limit had not been there, and that a second movement, made in imitation of the first, but without such an independent limit, would have a tendency to be too great. Eight hundred later experiments were made by the method of average error, upon two subjects, and care was taken to overcome this difficulty. The apparatus used in making them was more elaborate than that employed before, and was constructed partly with a view to discriminating between the force and the extent

of movements, and modifying either factor independently of the other.

This apparatus consists of a brass plate one meter long, graduated to millimeters, and grooved for the wheels of a small brass carriage. Along the scale is a wire, carrying an indicator (P) which is moved by a bar attached to the carriage. To the left of the experimentee, as he sits facing the apparatus, are two brass pins, which stop the carriage at the 0 point of the scale. Between the front and back wheels of the carriage, and parallel with the track, is a ring (R) into which is to be inserted the finger used in moving the carriage. Attached to the carriage is a cord (W) which runs over a pulley to the left of the experimentee, and admits of being weighted to any desired extent. Here, as before, the movement is from left to right. The indicator, which slides along the wire as the carriage moves, and remains at the farthest point reached, makes the readings much easier than in the apparatus used before, where the recorder had to catch the reading during the very brief time before the finger began the return movement. The carriage may be moved alone, or made to raise any weight attached to the cord. Thus one may study the effect of increasing or decreasing the amount of force demanded by movements of a given extent, and see to what degree judgments of distance are conditioned by the amount of force expended. So far, the writers have not found time to carry out such experiments.

SEC. 14. *Experiments by the Method of the Just Noticeable Difference.*—The method of the just noticeable difference has been described. It assumes that the smallest difference between two stimuli that can be consciously noted may be taken as a measure of the fineness of discrimination of any sense—the smaller the just noticeable difference, the greater the fineness of discrimination. But the use of this unit of measurement

is, as we have seen, not without its difficulties. We do not find that all things consciously noted are noted with equal clearness. We are clearly conscious of some things, less clearly conscious of others, and still others seem to be on the border-line between consciousness, or at least what usually passes by that name, and the obscure region which has been designated as "sub-consciousness" or "latent thought." And, just as there may be many degrees of objective difference below the level of the least degree that can be consciously noted as such by the unaided sense, so it is quite thinkable that there may be many corresponding degrees of sensation below the level of that which we are accustomed to regard as on the threshold of consciousness. Such sub-conscious mental modifications may differ from those commonly called conscious, simply as the fainter among these latter differ from the clearer. That such is the case, and that we have between the clearest consciousness and a complete cessation, if such there be, of all mental life a descending series, each member of which differs continuously from the one immediately preceding it, and a portion of which lies below what we commonly recognize as the threshold of consciousness, there seems good reason to believe. We do not have, then, two clearly marked divisions, the one lying below and the other above a well-defined threshold or ideal limit between sensations of quite different classes. Neither do we have a series beginning abruptly with what would be recognized as a "conscious" sensation.

If this be the case the just noticeable difference would seem to be a unit arbitrarily taken. Since we can, by varying the objective difference between two stimuli, bring about a varying degree of clearness in the consciousness of difference, the meaning of the words "just noticeable" must necessarily be vague. A larger difference than the one settled upon would be noted

with a clearer consciousness, and a smaller with a fainter, the differences between the cases being one of degree and not of kind. And although it is possible that the same observer may at different times give the words approximately the same meaning, there would appear to be no way at all of deciding that the just noticeable difference means the same thing in the case of different persons.

Again, the same degree of difference in the stimuli may, by the same observer, be perceived as a difference much more clearly at one time than at another. Many causes concur to produce at times a heightened consciousness and at times a less vivid one. Suppose that the difference between two weights is so small as to be distinguished with some clearness at one moment, and not thus distinguished at the next. Is it to be called a noticeable difference? If it be distinguished in nine cases out of ten, may we call it the *just* noticeable difference? Probably in several of those instances a smaller difference would have been noticed. It is this fluctuation in the power of discrimination, due partly to nervous changes in the observer, that has caused a series of experiments to be regarded as indispensable in the determination of the least noticeable difference.

The method of just noticeable difference may be applied in two somewhat different ways. Either two stimuli with a certain difference between them may be given the experimentee, and the difference increased or decreased until the observer considers it just noticeable—or a single stimulus may be given, and the experimentee required to fix upon another just noticeably greater or less. The just noticeable difference is here assumed to be the average obtained by combining the results of a number of experiments. In all the experiments given below as made by this method, the latter course was adopted. In this form, it will be seen that the method is analogous to that of average error;

and as the numbers of right and of wrong cases were recorded, the three psychophysical methods have been combined.

With the simple apparatus first described, 800 experiments were made by the method under discussion. Half of them were attempts to mark off on the scale a distance just greater, and half a distance just less than 500 mm. As details concerning the methods have already been given, it is only necessary to say that the recorder placed the movable upright at the 500 mm. mark on the scale, the experimentee moved his finger between the two uprights in the manner described, and then attempted, the movable upright having been taken away, to measure off a distance just greater or just less, as the case might be. The results of the experiments appear in the accompanying table.

TABLE II.

EXTENT OF MOVEMENT—METHOD OF JUST NOTICEABLE DIFFERENCE—F. EXPERIMENTEE—800 EXPERIMENTS—VII. VIII. 1890.

	JUST GREATER THAN 500 mm.				JUST LESS THAN 500 mm.			
	EXT.	V.E.	V.	% W.	EXT.	V.E.	V.	% W.
Av.	+ 36.6	11.4	5.7	3	— 24.7	10.	5.4	6
Var.	10.6	1.6	1.1		13.1	1.8	1.2	
Av.	+ 60.1	9.8	5.0	0	— 4.8	9.1	5.1	33
Var.	8.2	2.7	1.2		8.4	2.3	1.1	
Av.	+ 39.4	7.8	4.7	0	— 24.2	9.	5.3	4
Var.	5.0	1.6	1.1		7.2	1.8	1.1	
Av.	+ 21.5	8.9	4.7	5	— 37.7	8.6	5.1	0
Var.	6.3	2.	1.1		11.1	1.2	1.3	
Av. Av. . .	+39.4	9.5	5.	2	—22.8	9.2	5.2	10.7
Av. V. . .	9.2	2.	1.1		9.9	1.8	1.2	

Ext. The extent of the second movement as compared with 500 mm.

V.E. The average variable error made.

V. The mean variation of the variable error.

% W. The percentage of wrong cases; *i. e.*, attempts at greater which resulted in less, and *vice versa*.

Av. The average extent, variable error, and variation of the variable error for 100 experiments.

Var. The mean variation of the results of the ten sets of ten experiments each from their average as given above opposite *Av.*

Av. Av. The average of the four sets of figures opposite *Av.* above, giving the averages for 400 experiments.

Av. V. The average of the four sets of figures opposite *Var.* above.

As has been said, ten experiments of each of the eleven kinds made with the first apparatus were made at one sitting. The results of each such set of ten experiments have been calculated, and then these results combined in sets of ten to get the averages which appear in the four tables embodying the results of this investigation. The averages given in the tables consequently represent in each case 100 experiments. As 400 experiments of each kind were made, there are four such sets of averages for each kind of experiment, and these are combined at the foot of the tables in figures which represent averages for the whole 400 experiments. The averages are given in their chronological order, those at the top of each table representing the first hundred experiments made of the class indicated, those below them the second hundred, etc. The experiments made later with the second piece of apparatus were also made in sets of ten, and the results have been computed in the same way.

To enter more into detail, let the first set of averages in the table just given and under just greater than 500 mm. be taken. The figures to the right of *Av.* and under the headings *Ext.*, *V.E.*, *V.* and *% W.* indicate that, in a hundred experiments, the average distance measured off as just greater than 500 mm. was 536.6 mm.; the average variable error or mean variation of the individual distances from the average of each series of 10 experiments was 11.4 mm.; the mean variation of the variable error, or the average of the deviations of the variable error from its average, was 5.7 mm.; while three per cent. of the attempts to mark off a distance just greater than 500 mm. resulted in marking off a distance actually less.

The figures in italics below these averages have reference to the results of the separate sets of ten experiments each. For each set of ten experiments the average extent of the distance

measured off, the variable error or mean variation of the single distances from this average, and the mean variation of this variable error, were computed. The averages described in the preceding paragraph were, of course, obtained by taking the average of ten sets of results thus obtained. The mean variation from their average of these results representing in each case ten series appears in the second row opposite *Var.* Thus, the mean variation of the average distance measured off, in the first ten sets of ten experiments, from the 536.6, the figures obtained by averaging these averages, was 16.6 mm. Similarly the mean variation of the variable error for the ten sets was 1.6, and the mean variation of the variation of this variable error for the ten sets 1.1.

These explanations will apply, with a few obvious changes, to all the sets of figures on the table. Of course, where the endeavor was to measure off a distance just less than 500 mm., and the minus sign is used under the heading *Ext.*, it is meant that the distance measured off was as much less than 500 mm. as is indicated by the figures. For example, in the first block of averages under "just less than 500 mm.," the — 24.7 means that the average distance measured off in 100 experiments was 475.3 mm. The significance of the averages at the bottom of the table is too plain to need comment.

It appears, as a result of 400 experiments, that a movement, to be just noticeably greater than a previous movement of 500 mm. must be 539.4 mm. long; and from the same number of experiments, it appears that to be just noticeably shorter a movement must be 477.2 mm. long. The difference between the attempt at just less and the standard movement (5 per cent. of the former) is thus considerably less than the difference between the standard movement and the attempt at just greater (8 per cent. of the standard). A glance at the table will show, however,

that these results are modified by a marked tendency to overrate the standard in the second group of 100 experiments of both kinds. In these experiments the attempts at just greater gave an average of 560.1 mm., and the attempts at just less one of 495.2 mm., while 33 per cent. of the attempts at just less resulted in a movement actually greater than 500 mm. Still, in three out of the four groups of experiments of both kinds there appears to be a tendency to overrate the standard movement.

What has been said of the highly variable character of the just noticeable difference may be well illustrated from the table. Even for groups of 100 experiments it varies, for the attempt at just greater, between 60.1 mm. and 21.5 mm., and for the attempt at just less between 37.7 mm. and 4.8 mm. For the smaller groups of ten experiments the variation was still more marked. In the attempts at just greater it varied between 76 mm. and 9 mm., and in the attempts at just less between 49 mm. and 1 mm.

In striking contrast with these figures stands the very slight degree of fluctuation in the variable error and its mean variation. These would seem to be a much better measure of discriminative power than the least noticeable difference itself, for they do not appear to be subject to the disturbing influence of constants. For example, the average increment necessary to produce the consciousness of just greater in the second group of 100 experiments was 60.1 mm., and in the fourth group only 21.5 mm.; but the mean variations corresponding to these were respectively 9.8 and 8.9, and their variations in turn 5.0 and 4.7. Or, to take the second group of 100 experiments alone, as the tendency to overrate the stimulus was there most marked, while the increment under "just greater" was 60.1 mm., and the decrease under "just less" only 4.8 mm., their variable error was respectively 9.8 and 9.1, and the mean variation of this 5.0 and 5.1.

It is sufficiently evident that the just noticeable difference is of

little value in psychophysical experiments. It varies within wide limits even for sets of 100 experiments, and the only objective criterion of its significance—the percentage of right and wrong cases obtained—varies correspondingly. If the average of a large number of experiments be taken, the difference will be found to be too large, and the percentage of wrong cases too small to be satisfactory in experiments by the method of right and wrong cases. Where the proportion of right cases is so large, the results may easily be modified by accidental variations. Moreover, as the percentage of right cases does not increase uniformly as the difference increases, the average percentage of errors given in the table does not really correspond to the average just noticeable difference, but is somewhat too low. One may, it is true, apply the probability integral to the separate averages for 100 experiments, and compute the percentage of errors corresponding to the average just noticeable difference, but even in this case the results would not be wholly trustworthy, as the difference was highly variable even within the sets of 100 experiments. The variable error of the just noticeable difference furnishes a more satisfactory measure of discrimination, but there is no special advantage in taking the average variable error of this difference, as it is more regular when the method of average error is used.

SEC. 15. *Experiments by the Method of Estimated Amount of Difference.*—The method of mean gradation favored by Wundt¹ concerns itself only with the attempt to find a mean between two given stimuli (sensations?). There appears, however, no good reason for not using other relations as a measure of accuracy in discrimination. If one observer can fix more accurately than another upon a stimulus half as great, or twice as great, as a given one, his sense of the quantitative relations

¹ Phys. Psy., Bd. I. 8, § 1.

between stimuli must be finer. The method of estimated amount of difference would include all such estimates of the quantitative relations of differing stimuli.

Twelve hundred experiments were made by the method of estimated amount of difference. Of these 400 were attempts to mark off on the scale the half of 500 mm., 400 were attempts to double 300 mm., and 400 were attempts to find the mean between 300 and 700 mm. The details of the experiments were similar to those of the experiments by the method of just noticeable difference. In the attempt to halve 500 mm., the movable upright was placed by the recorder at the 500 mark on the scale, the experimentee moved his finger between the uprights, and then tried to make a movement half as long. In the same way a stimulus of 300 mm. was given, and then an attempt was made to double the distance; and, in the third case, two stimuli of 300 and 700 mm. were given one second apart, and the attempt made to give their mean in a third movement.

The results have been calculated and tabulated in precisely the same way as those of the experiments by the method of just noticeable difference, so that it is scarcely necessary to give a detailed description of the table. The four sets of averages for each kind of experiment represent in each case 100 experiments. *Var.* gives the mean variation from their average of the results obtained from the separate sets of ten experiments in each group of 100. Thus, to take the first 100 attempts to halve 500 mm., the figures on the first line in the table indicate that the average distance marked off was 305.8 mm., or 55.8 mm. too great; that the mean variation of the single distances from the average of each series of ten was 13.3, and the mean variation of the variations 7.5. The — 41.2 to the right of these in the same line indicates that the average distance measured off in the attempt to double 300 mm. was 558.8 mm., or 41.2 mm. too short. The

corresponding figures for experiments of the third kind show that the average of 100 attempts to find the mean between 300 mm. and 700 mm. resulted in a distance of 517.4 mm., or 17.4 mm. too great. The three sets of averages at the bottom of the table represent in each case 400 experiments.

TABLE III.

EXTENT OF MOVEMENT—METHOD OF ESTIMATED DIFFERENCE—F. EXPERIMENTEE—
1200 EXPERIMENTS—VII. VIII. 1890.

	HALVE 500 mm.			DOUBLE 300 mm.			MEAN 300 AND 700 mm.		
	EXT.	V.E.	V.	EXT.	V.E.	V.	EXT.	V.E.	V.
Av. . . .	+ 55.8	13.3	7.5	— 41.2	16.6	9.1	+ 17.4	15.2	8.2
Var. . .	13.4	2.5	1.1	23.4	4.6	2.3	15.5	3.8	2.4
Av. . . .	+ 71.9	12.3	6.7	— 18.1	14.4	7.7	+ 36.3	16.4	9.2
Var. . .	9.9	2.3	2.1	12.7	3.1	1.9	8.7	4.4	2.8
Av. . . .	+ 51.9	11.	6.6	— 42.3	11.5	7.1	+ 3.3	14.	8.9
Var. . .	10.8	3.2	2.	16.1	2.3	1.7	13.9	3.2	2.3
Av. . . .	+ 41.3	11.3	5.9	— 58.2	13.1	6.6	— 7.4	14.1	8.6
Var. . .	6.9	2.7	1.7	17.6	2.1	1.6	11.6	4.7	2.
Av. Av. .	+ 55.2	12.	6.7	— 39.9	13.9	7.6	+ 12.4	14.9	8.7
Av. V. .	10.2	2.7	1.7	17.4	3.	1.9	12.4	4.	2.4

As a result of 400 experiments of each kind, we find that the attempt to halve 500 mm. gave an average distance 55.2 mm. too long; the attempt to double 300 mm. a distance 39.9 mm. too short; and the attempt to find a mean between 300 and 700 mm. a distance 12.4 mm. too long. From the figures above these averages it is evident that the tendency toward the error they exhibit was fairly constant. In the attempts to halve 500 mm., not one of the forty sets of ten experiments gave an average that was not too great. In the attempts to double 300 mm., on the contrary, all but one set (1 mm. too great) gave an average that was too small. The experiments of the third kind were less constant. In three of the four groups of 100, the mean fixed upon was too great, but in only one of these groups

did each of the ten sets of ten experiments give an average too great. The final averages are considerably modified by the marked tendency to overrate the standard observable in the second 100 experiments of each kind. This tendency has been pointed out in discussing the corresponding groups of the experiments made by the method of just noticeable difference.

It is not easy to compare with each other the results obtained in the three kinds of experiments under discussion, for the experiments are quite different. It is, however, worthy of note that, whether we take the relation of the constant error made to the distance aimed at or to the stimulus given (in the experiments of the third class, to either of the stimuli), we find that the attempt to strike a mean between two stimuli results in a smaller error than the attempt to halve or to double a single stimulus. This is in harmony with the statement Wundt makes¹ in speaking of the measurement of mental intensities—namely, that the mean between two stimuli can be more accurately determined than can a stimulus bearing some given relation to a single stimulus. The difference does not, however, appear to be so great as his words would lead one to expect. Moreover, it should be kept in mind that the mean between 300 and 700 mm. is 500 mm., that this was one of the stimuli most constantly used, and that the objective quantitative relations of the stimuli were known to the experimentee. Before the first ten experiments of the kind in question were made, a stimulus of 500 mm. had been given the experimentee twenty times, and a but slightly different stimulus of 510 mm. had been given ten times. As a result of the experiments discussed in this monograph, the writers have been led to believe that the memory for absolute stimuli is better than has been commonly thought. Some experiments to show the memory of the experimentee for the stimuli employed

¹ Phys. Psy., Bd. I. 8, § 1.

in these experiments on the extent of movement will be given in a later section.

Furthermore, if we take, not the constant error, but the variable error or its variation, as a measure of the fineness of discrimination, what has been said of the accuracy with which a mean between two stimuli may be found does not hold. The 400 attempts to halve 500 mm. gave a variable error of 12; the attempts to double 300 mm. a variable error of 13.9; while the attempts at the mean between 300 and 700 mm. gave a variable error greater than either, 14.9. The average variation of the variable error for the forty sets of ten experiments in each case (in the table, under the figures just quoted) was, too, least for the experiments of the first kind, and greatest for those of the third, being respectively 2.7, 3.0 and 4.0.

The results of these experiments are all contrary to Fechner's law. If we assume that in such experiments we base our judgment on the quantitative relations of two sensations, and if, as Fechner maintained, the sensation increase as the logarithm of the stimulus, the attempt to halve 500 mm. should result in a distance less than 250, whereas it has given one of 305.2; the attempt to double 300 mm. ought to give a distance greater than 600, whereas it has given only 560.1; and the attempt to find the mean between 300 and 700 mm. should give a distance under 500, whereas the mean found is 512.4. That these results are not due simply to a constant tendency to over or underestimate the given stimuli, is evident from the fact that they are all contrary to Fechner's law, while any constant tendency to an over or underestimation of the stimuli would have the effect of making all the results greater or less together, and would not diminish the results of the experiments of the second kind, while increasing those of the first and third.

It will be noticed that in these experiments by the method of estimated amount of difference, the variable error is, in proportion to the whole extent of the movement made, greater than in the case of the experiments by the method of just noticeable difference. The attempts at just greater than 500 mm. resulted in a distance of 539.4, with a variable error of 9.5, or .018, and the attempts at just less in a distance of 477.2, with a variable error of 8.7, or .018 of that movement. In the three kinds of experiments now under discussion the movements were respectively 305.2, 560.1 and 512.4 mm., and the variable errors corresponding to them .039, .025 and .029. The attempt to double 300 mm. resulted in a variable error smaller in relation to the whole extent of the movement made than was the variable error in the experiments of the first and third kinds.

SEC. 16. *Experiments by the Method of Average Error.*—By the method of average error 1600 experiments were made with the apparatus first described, and 800 more were made later upon two different experimentees with the second piece of apparatus. Only the 1600 first made will be discussed in this section. They were of four different kinds, and consisted in attempts to measure off on the scale 100, 300, 500 and 700 mm. Thus, the recorder placed the movable upright at the 100 mm. mark on the scale, the experimentee moved his finger as described between the uprights, and then, the movable upright having been taken off the scale, attempted to mark off a distance equal to the standard. The same thing was done for 300, 500 and 700 mm. Four hundred experiments of each kind were made, and they were made, as were those described in the preceding sections, in sets of ten. The results have been counted out and tabulated in the same way as those already given. The four sets of averages for each kind of experiment represent in each case 100 experiments. The general averages at the bottom of the

table give the results of the whole 400 experiments of each kind. The figures in italics refer to the results obtained for the separate sets of ten experiments. To take again as representative a single block of figures: the +8.9, in the first line under 100 mm., and opposite Av., signifies that the first hundred attempts to repeat a movement of 100 mm. resulted in an average movement of 108.9 mm., or one 8.9 mm. too long; the 5.7 in the same line gives the mean variation of the single distances from the average of each series of ten; and the 3.5 gives the mean variation from its average of this variation. In the second line, the 5.1, .6 and .6 give the mean variations of the results obtained for the ten separate sets of ten experiments from their respective averages as indicated in the line above.

TABLE IV.

EXTENT OF MOVEMENT—METHOD OF AVERAGE ERROR—F. EXPERIMENTEE—1600
EXPERIMENTS—VII. VIII. 1890.

	100 mm.			300 mm.			500 mm.			700 mm.		
	EXT.	V.E.	V.	EXT.	V.E.	V.	EXT.	V.E.	V.	EXT.	V.E.	V.
Av. . .	+ 8.9	5.7	3.5	+ 3.7	8.8	4.6	+ .8	10.8	6.2	+ 2.8	10.4	6.1
Var. . .	<i>5.1</i>	<i>.6</i>		<i>9.6</i>	<i>1.6</i>	<i>1.0</i>	<i>7.4</i>	<i>1.8</i>	<i>1.2</i>	<i>10.8</i>	<i>2.8</i>	<i>1.7</i>
Av. . .	+ 18.4	5.3	3.1	+ 21.4	9.5	5.5	+ 27.6	10.8	6.3	+ 13.1	7.7	4.9
Var. . .	<i>3.1</i>	<i>1.1</i>	<i>.5</i>	<i>9.0</i>	<i>1.9</i>	<i>1.7</i>	<i>5.6</i>	<i>2.8</i>	<i>1.3</i>	<i>4.7</i>	<i>2.1</i>	<i>1.3</i>
Av. . .	+ 14.7	5.3	2.9	+ 2.4	7.3	4.6	+ 3.0	8.3	4.3	— 6.4	9.4	5.7
Var. . .	<i>2.9</i>	<i>1.7</i>	<i>.7</i>	<i>8.6</i>	<i>2.4</i>	<i>1.6</i>	<i>6.2</i>	<i>2.2</i>	<i>1.1</i>	<i>7.8</i>	<i>2.0</i>	<i>1.2</i>
Av. . .	+ 5.4	4.8	2.8	— 16.4	9.5	5.2	— 14.0	8.2	5.4	— 28.9	8.	4.9
Var. . .	<i>3.6</i>	<i>.8</i>	<i>.6</i>	<i>9.6</i>	<i>2.1</i>	<i>.8</i>	<i>8.6</i>	<i>2.0</i>	<i>1.4</i>	<i>9.3</i>	<i>2.0</i>	<i>1.7</i>
Av. Av. .	+ 11.8	5.3	3.1	+ 2.8	8.8	5.0	+ 4.3	9.5	5.5	— 4.8	8.9	5.4
Av. Var.	<i>3.7</i>	<i>1.</i>	<i>.6</i>	<i>9.2</i>	<i>2.</i>	<i>1.3</i>	<i>6.9</i>	<i>2.2</i>	<i>1.2</i>	<i>8.1</i>	<i>.22</i>	<i>1.5</i>

From the table it appears, in each case as a result of 400 experiments, that the attempts to mark off 100, 300, 500 and 700 mm. gave an average of, respectively, 111.8, 302.8, 504.3 and 695.2 mm. The tendency to overestimate the shortest distance, 100 mm., was very marked. The greatest distance, 700 mm.,

was underestimated. There is a general agreement between these results and those of the experiments made with the second piece of apparatus, as will be pointed out in the next section; and it will be seen, when the experiments made on the force of movement are described, that a similar tendency is apparent. Evidently the constant error, as it stands here, cannot be taken alone as a measure of the fineness of discrimination in judging of the extent of movements. It is much the greatest in the case of the smallest movement.

The variable error increases as the distance increases, until we come to the 700 mm. movement, when it falls somewhat. For the four movements it is respectively 5.3, 8.8, 9.5 and 8.9; or

	For 100 mm.	300.	500.	700.
Compared with the standard movement, }	.053	.029	.019	.013
Compared with the movement actually made, }	.047	.029	.019	.013

Thus the variable error does not accord with Weber's law, but increases much more slowly than the stimulus. Its relation to the square root of the stimulus is for the four movements :

For 100 mm.	300.	500.	700.
.530	.513	.424	.336

It increases, therefore, somewhat more slowly than the square root of the stimulus, for the first three movements, and falls considerably with the last.

If this variable error be taken as a measure of discriminative power, it would seem that a distance of 500 mm. is not as accurately discriminated as one of 700. It should be taken into consideration that, in making such movements as those employed in these experiments, very different muscles are brought into play at different times, and the tension upon the skin and joints is much greater in the more extended movements. Placed as the

experimentee was, before the 100 mm. mark on the scale, there was a distinct feeling of strain in reaching to the 700 mm. point, although the finger could readily be extended some distance beyond it. It is not inconceivable that the peculiar factors entering into the consciousness of a movement of 700 mm. should make discrimination finer for such a movement than for one of 500 mm. A similar fall of the error with the last movement will be seen in the results of the later experiments to be described below.

The amount of the variable error, of its variation, and of the constant error, for each of the four stimuli selected may be seen at a glance from the accompanying curves.

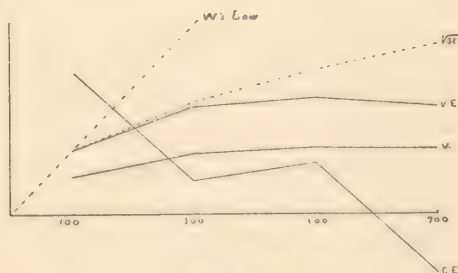


FIG. 3.

V.E. Curve representing the amount of the variable error for 100, 300, 500 and 700 mm. as determined for each point by 400 experiments by the method of average error. F. experimentee.

V. Similar curve representing the mean variation of the above variable error.

C.E. Similar curve representing the amount of the constant error.

W's Law. A line representing the increase of the variable error according to Weber's law, the smallest magnitude being taken as norm.

Vst. Curve representing the increase of the variable error as proportional to the square root of the stimulus.

It is interesting to compare the variable error obtained in these experiments with that obtained in the experiments by the methods of just noticeable difference and estimated amount of difference, when the movement was approximately the same. In

the experiments by the method of just noticeable difference, with movements of 477.2 and 539.4 mm., the error was .018; in those by the method of estimated amount of difference, with the movement of 512.4, the error was .029; in these experiments, with a movement of 504.3, the error was .019. Again, in the experiments by the method of estimated amount of difference, with a movement of 305.2, the error was .039; in these experiments, with a movement of 302.8, the error was .029.

We find, therefore, that the variable error is greatest in experiments by the method of estimated amount of difference—a result one would naturally expect, in view of the difficulty of holding such relations in mind during the experiments.

It may be well to insert here the record of a few attempts made to mark off from memory, and without previous stimulus being given, the distances used as stimuli in the experiments on the extent of movement. After twenty complete series of experiments had been made, that is, after one-half of the whole number, the experimentee tried to measure off ten times in succession a distance of 100, then one of 300, of 500 and of 700 mm. After forty series—the whole number of experiments—this was done again, but then done three times, making thirty attempts at each of the distances. The results of the forty trials at each of the four stimuli are as follows:

TABLE V.

	100 mm.		300 mm.		500 mm.		700 mm.	
	EXT.	V.E.	EXT.	V.E.	EXT.	V.E.	EXT.	V.E.
After 20 .	+ 34	14	+ 48	21	+ 37	25	+ 11	10
After 40 .	+ 22	10	— 7	31	— 5	17	— 10	14
	+ 20	12	+ 1	13	— 20	13	+ 8	15
	+ 19	8	— 9	14	— 29	21	— 16	16
Av. . . .	+ 24	11	+ 8	20	— 4	19	— 7	14

Thus forty attempts to measure off without previous stimulus 100 mm. resulted in a distance 24 mm. too great; as many to measure off 300 gave a distance 8 mm. too great; the same number for 500 and 700 gave distances too short by, respectively, 4 and 7 mm. There is evidence of the tendency before remarked to overrate the shorter distance. The variable error is in every case greater than in the corresponding division of the experiments by the method of average error, where previous stimulus was given. The error falls, as it there does, with the last movement, indicating a greater accuracy of discrimination.

These experiments show that our memory for absolute distances is fairly accurate, and they are of some significance in connection with the experiments by the method of estimated amount of difference. The attempt to find a mean between 300 and 700 mm. resulted in a constant error of $+12.4$; this mean was known by the experimentee to be 500; the attempt to mark off 500 mm. from memory gave a constant error of -4 only. It follows that the smallness of the constant error in the attempts to find the mean between 300 and 700 mm. proves little in regard to the accuracy with which a mean can, in general, be found between two stimuli. On the other hand, the fact that the variable error in these attempts to find a mean was .029 of the movement made, while the variable error in the memory-experiments was .038 of the movement made, indicates that the presence of the stimulus had the effect of heightening the accuracy of the judgments.

The amount of the variable error, in the experiments by the method of average error, when previous stimulus was given, was, as has been said,

Compared with the standard movement,	}	For 100 mm.	300.	500.	700.
		.053	.029	.019	.013
Compared with the move- ment actually made,	}	.047	.029	.019	.013

In these experiments it is

	For 100 mm.	300.	500.	700.
Compared with the movement aimed at,	.110	.067	.038	.020
Compared with the movement actually made,	.089	.065	.038	.022

It is thus about double the error obtained with previous stimulus given. Its excess over this would seem to indicate the variable error of memory.

The amount of this error, and of the constant error for each of the four distances, is represented in the accompanying curves. They represent the amount of the error as compared with the distance aimed at, and not as compared with the movement actually made. As they are drawn on the same scale as the former curves, their divergence from these may be readily seen.

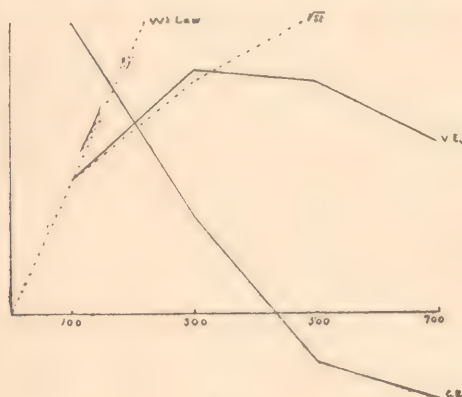


FIG. 4.

SEC. 17. *Later Experiments by the Method of Average Error.*

—The 800 experiments by the method of average error with the second piece of apparatus were made upon two subjects—400 upon each. The distances taken as stimuli were, as before, 100, 300, 500 and 700 mm. The details of the experiments

were similar in the two cases, except that in the later experiments the stimulus was not determined by the two uprights, but was set by the experimentee himself. Before each set of ten experiments with the one stimulus, the experimentee was told to measure off the distance chosen as stimulus. He was then informed whether the distance he had measured off was too great or too small. Four more trials were allowed him, after each of which he was informed of the nature and amount of his error, and he was then required, in making the experiments, to determine the extent of the first movement by the standard thus held in memory. The second movement was an attempt to repeat the first. It will be seen that the conditions of the first and second movements were, in this case, similar, neither of them being terminated by a fixed limit.

TABLE VI.
EXTENT OF MOVEMENT—METHOD OF AVERAGE ERROR—K. AND D. EXPERIMENTEES—800 EXPERIMENTS—1890.

	100 mm.				300 mm.				500 mm.				700 mm.			
	EXT. I.	EXT. II.	AV. E.	V. E.	EXT. I.	EXT. II.	AV. E.	V. E.	EXT. I.	EXT. II.	AV. E.	V. E.	EXT. I.	EXT. II.	AV. E.	V. E.
K.	105.9	+ 3.8	11.9	11.4	307.8	+ 0.2	21.8	21.1	506.3	— 3.2	25.8	22.6	692.9	— 0.7	21.0	16.9
Var.	8.8	5.6	2.1	2.0	21.2	10.0	4.6	5.1	15.6	11.0	7.4	6.5	13.0	13.8	5.9	3.7
D.	101.0	— 2.9	17.3	16.2	301.1	— 14.3	32.4	26.1	473.9	— 24.1	41.3	31.3	697.7	— 19.7	33.5	25.7
Var.	5.6	4.3	2.8	3.7	14.7	10.0	5.1	7.1	14.9	10.1	6.3	5.5	15.6	13.9	10.2	7.5
Av. Av. . . .	103.4	+ 0.4	14.6	13.8	304.4	— 7.0	27.1	23.6	490.1	— 13.6	33.5	26.9	695.3	— 10.2	27.2	21.3
Av. Var. . .	7.2	4.9	2.4	2.8	17.9	10.0	4.8	6.1	15.2	10.5	6.8	6.0	14.3	13.8	8.0	5.6

These experiments were, unfortunately, not made on *F.*, the subject in the earlier experiments, and one cannot be sure that the difference in the constant error is due to the difference in the conditions under which the experiments were made. Still, it is interesting to note that in these experiments there is a tendency to diminish the relative extent of the second movement, and it is not improbable that the absence of the upright, which, in the earlier experiments, limited the first movement, has something to do with this.

The tendency to underestimate the standard movement, as the distance increases, is here, as before, sufficiently marked. For the first three movements the constant error varies nearly as the movement. With the last movement it diminishes.

Neither the average error nor the variable error increases directly as the extent of the movement, and hence they do not agree with Weber's law. For both subjects they increase more nearly as the square root of the distance for the first three movements, though the approximation is not very close. For both subjects these errors fall decidedly with the last movement.

Whether, therefore, we consider the constant error, the variable error or the average error, we find that discrimination is finer for a movement of 700 mm. than for one of 500. This accords with the results of the earlier experiments, as far as the variable error is concerned. In those experiments, however, the constant error does not follow the course it does here. It falls, then rises, then falls again to a negative error rather greater than the positive error at 500 mm.

It will be seen that, though the results obtained with the two subjects are of the same general nature, all three errors are much greater in the case of the second subject. This would indicate a greater fineness of discrimination in the first, who is a woman. Whether women generally have a finer discrimination in judging

of the extent of movements would have to be determined by experimenting on a considerable number of persons. A comparison of the variable error obtained in the experiments on K . with that obtained in the earlier experiments on F . will show that F 's error is smaller for all four points. F 's constant error, however, is larger.

Curves indicating the amount of the constant error, the average error, and the variable error for K . and D ., as determined by 100 experiments for each point, as also curves giving the average for the two subjects, are subjoined.

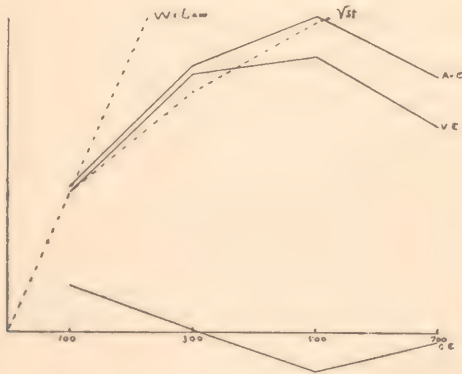


FIG. 5.—CURVES FOR K .

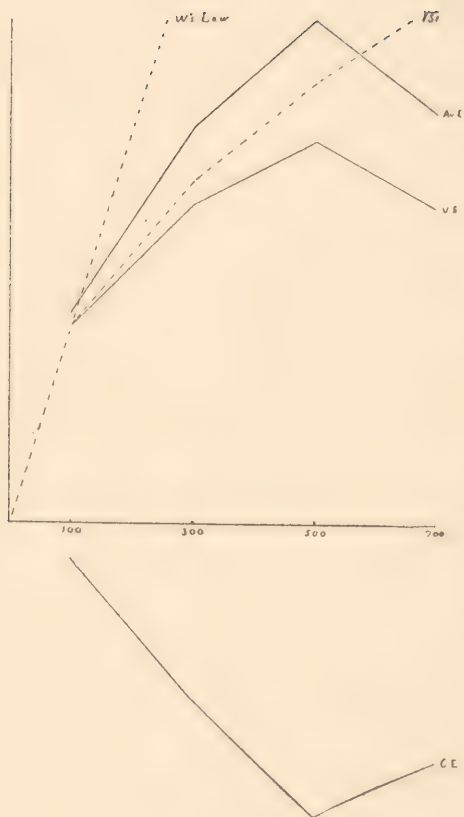


FIG. 6.—CURVES FOR D.

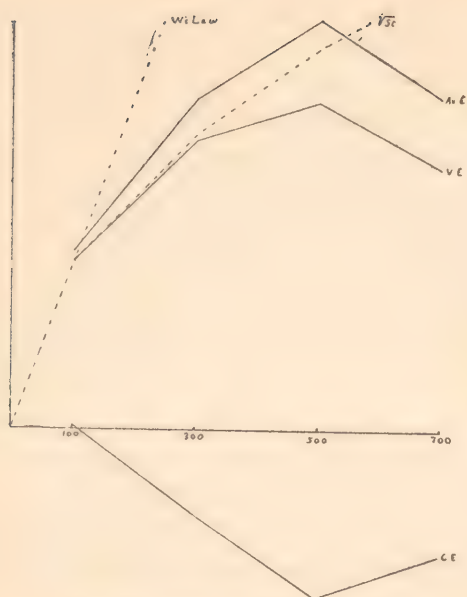


FIG. 7.—AVERAGE FOR K. AND D.

The amount of the average and variable errors is thus, in relation to the distance aimed at:

	For 100 mm.	300.	500.	700.
Average error for K.,	.119	.073	.051	.030
“ “ D.,	.173	.108	.083	.048
Variable error for K.,	.114	.070	.045	.024
“ “ D.,	.162	.087	.063	.037

If the error be compared with the distance actually measured off, the figures are but little changed. The *abscissæ* in the curves indicate the distance aimed at. The relative amount of the constant error in figures might be misleading, as it passes from + to —.

SEC. 18. *Experiments by the Method of Right and Wrong Cases.*—By the method of right and wrong cases 800 experi-

ments were made, the stimuli used being 500 and 510 mm. The method has been described and criticised in the introduction, and it is not necessary to dwell upon it here. Most of the needful details concerning the experiments have already been given. Briefly stated, the experiments were made as follows: The movable upright was placed by the recorder at the 500 or the 510 mm. mark on the scale, as the case might be. The experimentee then moved his finger between the uprights, and thus obtained the standard movement. As he recovered position for a second movement, the movable standard was shifted to the 510 or the 500 mm. mark. He again made a movement between the two uprights, and then gave a decision as to whether the second movement was longer or shorter than the first. In giving his decision he used the letters *a*, *b* or *c* to express his degree of confidence in the correctness of this judgment, *a* indicating that he was quite confident, *b* that he was fairly confident, and *c* that he was less confident.

Twenty experiments by this method were made at a sitting; in ten of them the second stimulus was the greater, and in ten the less. Of course, the second stimulus was made greater or less at random, and care was taken to avoid following any order which could be surmised by the experimentee. The twenty experiments were all made, as were the sets of ten in the other kinds of experiments, before taking up another kind. The results of the experiments are given in the accompanying table.

TABLE VII.

EXTENT OF MOVEMENT—METHOD OF RIGHT AND WRONG CASES—500:510 mm.—
F. EXPERIMENTEE—800 EXPERIMENTS—VII. VIII. 1890.

	2D THE GREATER.			2D THE LESS.			AVERAGE.		
	R.	W.	D.	R.	W.	D.	R.	W.	D.
1st Group	76	21	3	75	22	3	75.5	21.5	3
2d "	67	32	1	71	28	1	69	30	1
3d "	81	19	0	67	33	0	74	26	0
4th "	76	24	0	74	26	0	75	25	0
Average	75	24	1	71.8	27.2	1	73.4	25.6	1
% of whole number of right cases when the 2d was greater									51.1
" " " wrong " " " " "									46.9
DEGREE OF CONFIDENCE.						a.	b.	c.	d.
% of times used						.5	12.5	86.	1.
" " " when right						.7	15.1	84.2	
" " " wrong						.0	5.1	94.9	
" " right in the case of each degree						100	97.	71.7	

R, Right.

W, Wrong.

D, Doubtful.

First group representing 100 experiments of each sort, as does each of the other groups.

Average, The average of 400 experiments.

a, Quite confident.

b, Fairly confident.

c, Less confident.

The 400 experiments in which the second movement was greater, and the 400 in which it was less, are each divided into four groups of 100 experiments, and the percentages, which are also the numbers of right, wrong and doubtful judgments, are given for each group, under *R*, *W*, and *D*. The groups are arranged in chronological order. Thus, in the first 100 experi-

ments made with the second movement greater, it was judged greater seventy-six times, less twenty-one times, and the experimentee was unable to come to a decision three times. In the first 100 with the second movement less, it was judged less seventy-five times, greater twenty-two times, and three times no decision was arrived at. Combining these two sets of figures in an average we get (in the table, to the right of the figures just given) 75.5 per cent. of right judgments, 21.5 per cent. of wrong ones, and three per cent. of cases in which the experimentee was in doubt. Below the averages for the groups of 100 are the averages for the four groups of each kind. To the right of these is their average, which, consequently, represents 800 experiments.

The percentage of the whole number of right cases when the second movement was greater (51.1) is obtained, of course, by adding the two averages 75 and 71.8, and dividing 75 by their sum. The percentage of the whole number of wrong cases when the second movement was greater (46.9) is similarly obtained from the averages 24 and 27.2.

As regards the degree of confidence expressed, the figures in the table are sufficiently clear. They have reference to the whole 800 experiments. Thus, the experimentee was quite confident he was right only 0.5 per cent. of the time; he was fairly confident 12.5 per cent., less confident still 86.0 per cent., and wholly undecided 1.0 per cent. To pass to the second line, he was quite confident he was right 0.7 per cent. of the whole number of times in which he was right; he was fairly confident 15.1 per cent., and less confident 84.2 per cent. The third line of figures so resembles the second as to need no comment. The fourth shows to what degree the subjective feeling of confidence corresponded with objective correctness in the judgment. It indicates that when the experimentee was quite con-

fidant he was right, he was always right ; when he was fairly confident, he was right 97 per cent. of the time ; and when he was less confident, he was right 71.7 per cent. of the time.

Thus it appears that with the two stimuli 500 and 510 mm., 75 per cent. of the judgments were right when the second stimulus was the greater, and 71.8 per cent. were right when the second was the less. This would indicate that the first movement was slightly underrated on the whole. In the earlier experiments by the method of average error a standard movement of 500 mm. was slightly overrated. In the later experiments it was underrated. The second group of the experiments by the method of right and wrong cases differs from the other three in showing an inclination to overestimate the standard. It will be remembered that the corresponding groups in the experiments by each of the other three methods showed a similar tendency. The ten series of experiments represented by these groups were made within a period of seven days. During this time the standard movement seems to have been pretty constantly overrated.

As in the criticism of the just noticeable difference as a standard of measurement it was objected that the significance of the words "just noticeable" is not easily determined, so with regard to the degrees of confidence *a*, *b*, and *c*, it may be objected that the terms "quite confident," "fairly confident" and "less confident" are extremely vague. In a series of experiments with the one observer each of these terms may be assumed, perhaps, to have approximately the same meaning in different parts of the series ; but the quantitative relations of the subjective feeling of confidence in the three cases remain very obscure, nor can it be assumed that they may be measured by the percentage of right cases corresponding to each degree of confidence. The fact that an observer is always right when

he feels quite confident, and right ninety-seven per cent. of the times when he feels fairly confident, does not prove that the amount or intensity of his confidence in the two instances is as 100 to 97. We see, however, from the figures in the table that the observer was sure to be right when he felt confident enough to say *a*, nearly sure to be right when he was willing to say *b*, and much less likely to be right when moved to say *c*.

The average variable error obtained with a stimulus of 500 mm. in the experiments by the method of average error was 9.5. Now, if we eliminate in these experiments the constant error (.7), and calculate the average error (that corresponding to 78 per cent. of right cases) by applying the probability integral to the figures in the above table, we obtain an error of 12.1. The average variable error in the experiments by the method of just noticeable difference was, for just greater than 500 mm., 9.5, and for just less 8.7. If, by the use of the probability integral, we calculate from the just noticeable difference (Table II.), the difference with which the observer should be right in his judgment seventy-eight per cent. of the time, we obtain a difference of 12.9, which agrees very well with the results obtained by the method of right and wrong cases. But it is evident that the several series vary more, and that the probable error of the result is greater.

PART II.

ON THE FORCE OF MOVEMENT.

SEC. 19. *Introductory.*—The experiments hitherto made on the so-called “muscular sense,” or “sense of innervation,” have been carried out with weights. We also began with such experiments (which are described in Part IV.), but the great advantage of using a dynamometer soon occurred to us. With a dynamometer the several psychophysical methods may be used and compared, and the factors concerned in the perception of movement may be distinguished and studied separately. If lifted weights be used as stimuli, the only method which can be applied conveniently is that of right and wrong cases. It is not possible to make one weight equal to another, just heavier or twice as heavy, without repeated trials, and if regularity be observed the method of right and wrong cases must be introduced as a secondary help. The method of right and wrong cases is indeed admirably suited to the discrimination of lifted weights, as the magnitude of the stimulus can be measured without appreciable error. But this method, as we have seen, requires numerous experiments before conclusive results can be reached.¹

Further, it is not easy to analyze the factors concerned in the perception of lifted weights. We might expect the lighter

¹ Fechner heroically made 67072 trials with lifted weights, and yet considered these too few. Galton (*Inquiries into Human Faculty*, London, 1883, Appendix D) suggests that the influence of chance may be lessened by allowing the observer to arrange three weights in order. But the observer might distinguish the heaviest weight from the lightest and place the third between.

weight to be lifted higher and faster than the heavier, and the judgment of the observer might depend on the perception of the force, of the extent or of the time. Müller and Schumann¹ conclude that the observer's judgment is due chiefly to the time, and explain Weber's law by the principle of mechanics, according to which a proportional increase in force is required to cause an equal increase in velocity. Our experiments prove the incorrectness of this ingenious theory, as they show that the force of a movement can be judged better than its time, and that the judgment of time follows Weber's law more nearly than the judgment of force.

SEC. 20. *Apparatus and Methods.*—The clinical dynamometers in use are too inaccurate for scientific experiment.² We therefore constructed a special dynamometer for our work. This is shown in Fig. 8.

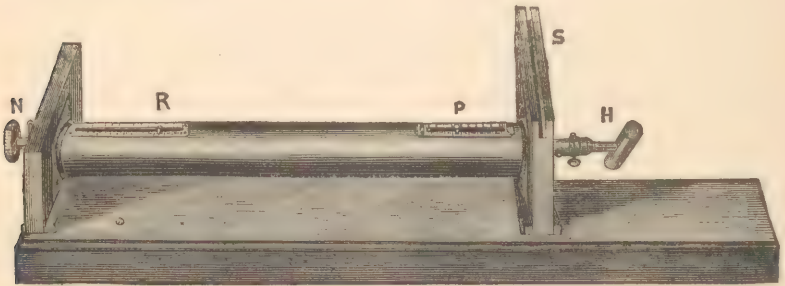


FIG. 8.—APPARATUS FOR MEASURING THE FORCE OF MOVEMENT.

A heavy spiral spring is enclosed in the brass cylinder (*R P*) to which the handle (*H*) is attached by a bar. The bar runs on

¹ G. E. Müller und Fr. Schumann. Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte. *Pflüger's Archiv*, XLV. 1889.

² We tested an oval (Collin) dynamometer and found the absolute readings to be worthless, while the average error of adjustment was very large. A reputable instrument-maker informed us that he did not know whether the reading on his dynamometers gave lbs. or kg. He made the springs, and, without testing them, copied the scale from a French dynamometer. The scientific value and practical usefulness of clinical observations would be much increased by more accurate methods and instruments.

double wheels almost without friction. When the handle is pulled out the amount of force applied is shown on the scale (P), as in an ordinary spring balance. The pointer, however, not being attached to the bar, is only pushed forward, and stays at the point of the maximum pull. The recorder can thus take the exact reading before replacing the pointer. The scale registers up to 10 kg., and in addition a second scale (R) is attached, registering 15 kg. further. By means of the bar, pivot and screw (at N) the spring can be set at any point up to 15 kg. In such a case, the observer must pull the set amount before the handle move, while the force of his pull beyond this amount is registered on the front scale. The advantage of a double scale is twofold. The maximum force of movement may be kept the same, while the extent and rate are altered, and the total extent of the movement may be made as small as desired.¹ The instrument was tested carefully, and the errors of adjustment and of reading were small as compared with the quantity measured. The force of pull was varied from 2 to 16 kg., and the reading was taken to .01 kg.²

The movement used in our experiments was a free pull with the arm. The observer sat on a high chair with his right shoulder opposite the instrument, which was fastened to an ordinary table. The handle was conveniently grasped, always in

¹ In some clinical dynamometers the amount of movement is so great that the grasp with which the observer begins must be altered while the movement is being made; if, on the other hand, the amount of the movement be less, it is too small to be accurately registered. In our dynamometer there is an attachment which may be fastened to the extension (below H), if it be desired to use the grasp of the hand, or the pressure of the thumb and forefinger. The range (25 kg.) of this dynamometer is suitable for the latter movement, which we recommend for clinical observation, as it is less liable to accidental alterations of position than the grasp of hand.

² The error of reading would not increase as the magnitude of the stimulus is taken greater, and would form a larger part of the (apparent) error of observation when small than when great. It is, however, so small a part of the total error in these experiments that it may be disregarded. The error of the instrument and possible errors of calculation are more serious complications, but we should not know how to allow for these.

the same manner. A natural pull was made, the muscular mechanism being different for pulls of different force, but kept as nearly as possible the same for the same pull. A screen (attached in *S*) hid the scale and record-book from the observer. We had intended to give the observer the first or normal force of pull by an adjustable stop which can be attached to the extension (below *H*), and removed before the second or judgment pull be made. We saw, however, that some part of the force of the first pull would be spent in striking the stop, which led us to adopt the method described in Part I., which we believe to be a great advantage in all psychological experiment in which it can be used.

The observer was told to give a pull of (say) 2 kg. As might have been expected, his error in estimating a standard magnitude was usually very great. He was then told the direction and approximate amount of his error and allowed to try again. This was repeated until he had made five trials, by which time he could usually give the standard without great variation. A series of ten judgments was then made, the observer giving in each trial first the standard pull from memory, and then a pull as nearly as possible (say) equal to it. A series of five trials preceded each series of ten judgments, and if in the course of the series the first pulls varied greatly from the standard, the observer was told to make his pulls less or greater as the case might be. The observer was told not to attend particularly to the amount of the standard, but to concentrate his attention on the comparison; and the error of his observation, as discovered in the difference of the stimuli, was made the basis of the averages. With this method the observer produces and perceives both stimuli under like conditions, and the comparison can be made to great advantage.

The time conditions were made as constant as possible. A

pendulum, giving seconds, swung in front of the observer, and after a little practice its movement could be followed automatically. The first or standard pull was made at the beginning of a second and lasted about one second, a second was allowed for recovery, and the judgment pull was made during the third second. The stronger pulls lasted, approximately, one second, while the weaker pulls were made in a little less time, the observer being allowed to make them in the manner which he found most convenient. He was expected to make his decision during the fourth second. If we had been able to continue our work, we should have measured these times by means of a chronoscope, so as to discover more exactly the relations between force, extent and rate. We believe that interesting results would be disclosed by such a research.

When 2, 4 and 8 kg. were used as stimuli, the extent of the pull was proportional to the force. When a stimulus of 16 kg. was used, the spring was set at 10, and the handle began to move when the force was 10, and the distance it moved was proportional to the force beyond 10. The handle moved 6.4 mm. for each kg. If time had permitted, we should have used dynamometers in which the extent would have been kept constant, and also those in which it would have varied otherwise than in direct proportion to the force. We believe the relations between these magnitudes to be an interesting subject for research. The present experiments on the force of movement are complicated by the variation in extent. The observer's attention was concentrated on the force, and casual introspection indicated that the difference in force only was regarded. More careful introspection (taken in connection with the accuracy with which the extent of movements can be judged, as shown in Part I.) seemed, however, to indicate that the observer was helped more by the variation in extent than he had at first supposed. As the extent varied

directly with the force for 2, 4 and 8 kg., any general relation (such as Weber's law) between the error of observation and the magnitude of the stimulus would not be affected. With 16 kg., however, the extent was to the force as 6:16. If the observer therefore judged partly by the extent, the error for 16 kg. would be relatively smaller than if he judged only by the force.

Most of the experiments (4000 judgments) on the force of movement were made by *J.* and *C.*, who acted alternately as observer and recorder. They did not know any results of their own experiments until the whole series had been completed, and but little concerning the results of the other observer, as these were not calculated while the work was in progress. A series of experiments with the several magnitudes and by the different methods was made together, the order being altered in each series, so as to eliminate, as far as possible, the effects of fatigue and contrast. By the method of average error, 400 experiments each were made on *F.*, *D.* and *K.*, *C.* or *F.* acting as recorder.

SEC. 21. *Experiments by the Method of the Just Noticeable Difference.*—In these experiments a standard pull, 2, 4, 8 or 16 kg., was fixed in mind by a series of five trials, and then a series of ten judgments was made. The observer gave the standard, and then a pull which appeared just greater (or less). We have already considered the difficulties in the way of making experiments by the method of the just noticeable difference. In these experiments the observer did not attempt to make the difference in the pulls so great that he was sure that he was right in the objective relation of the magnitudes, but so great that he felt tolerably confident in the correctness of his judgment. He would have guessed himself to have been right in his judgment about nine times out of ten, the confidence corresponding to that marked

"*b*" in the method of right and wrong cases. If the observer made the difference greater than he had intended, or for some other reason felt sure that his judgment was correct, this experiment was marked "*a*;" if, on the other hand, he felt uncertain as to the correctness of his judgment, the experiment was marked "*c*." It would have been more accurate to have treated these different classes of judgment separately, but as "*a*" and "*c*" seldom occurred, we did not undertake the additional calculation which this would have involved.

The results of the experiments by this method are given in Table VIII., which is arranged in a manner analogous to those in Part I.

TABLE VIII.

FORCE OF MOVEMENT—METHOD OF THE JUST NOTICEABLE DIFFERENCE—J. AND C.—1600 EXPERIMENTS—IX. 1890—I. 1891.

2 KG. = 200.										4 KG. = 400.										8 KG. = 800.										16 KG. = 1600.									
I.		V.		II.		V ¹ .		V. E.		W.		I.		V.		II.		V ¹ .		V. E.		W.		I.		V.		II.		V ¹ .		V. E.		W.					
J. . .	205	15	+ 34	17	13.1	3	389	20	+ 49	26	20.	7	781	43	+ 55	48	35.8	12	1608	51	+ 56	47	53.5	27	1608	51	+ 56	47	53.5	27	1608	51	+ 56	47	53.5	27			
Var. .	8	6	7	6	4.1	29	29	8	13	8	5.2	31	14	17	15	15.6	56	15	35	16	13.1																		
C. . .	209	21	+ 65	20	17.3	1	389	30	+ 84	34	33.5	3	789	52	+ 91	48	37.5	5	1609	40	+ 79	45	37.3	5	1609	40	+ 79	45	37.3	5	1609	40	+ 79	45	37.3	5			
Var. .	7	7	10	7	4.3	15	15	8	15	8	8.7	31	10	23	11	8.3	32	10	20	9	5.1																		

ATTEMPT AT JUST GREATER.

J. . .	210	14	— 20	13	10.8	10	400	24	— 31	26	19.7	17	778	34	— 72	36	29.2	8	1617	40	— 89	49	39.6	9	1617	40	— 89	49	39.6	9	1617	40	— 89	49	39.6	9
Var. .	10	4	8	3	2.8	32	32	8	21	5	7.1	43	8	28	7	7.8	56	10	24	10	10.															
C. . .	215	18	— 36	18	20.	7	383	31	— 54	25	26.8	11	812	45	— 142	42	38.9	5	1646	49	— 133	34	34.9	2	1646	49	— 133	34	34.9	2	1646	49	— 133	34	34.9	2
Var. .	18	4	11	7	3.8	18	18	6	17	9	7.	39	10	36	11	10.9	41	19	28	16	4.7															
Av. . .	210	17	± 39	17	15.3	5	2390	26	± 54	28	25.	9.5	790	43	± 90	43	35.3	7.5	1620	45	± 89	44	41.3	10.7	1620	45	± 89	44	41.3	10.7	1620	45	± 89	44	41.3	10.7

ATTEMPT AT JUST LESS.

We think the table will be readily understood. The unit used in the tables and text is .01 kg. The results for the two observers, *J.* and *C.*, and for "just greater" and "just less" are given separately. The basis of the figures opposite *J.* and *C.* is ten series, each containing ten judgments, while the average is the result of the 400 experiments with each magnitude. The figures in italics opposite *Var.* give the average variations of the ten series. This variation is very nearly proportional to the probable error of the average, which is about $\frac{.845}{\sqrt{n-1}}$ or $\frac{1}{4}$ (more nearly .282), the variation given in the tables. Under I. is given the average force of the first or standard pull, and under II. the average amount of difference between the two pulls, together with its direction—that is, the "just noticeable difference." Under *V.* and *V.*¹, respectively, are given the average variations of the pulls from the series to which they belong. These variations are dependent on memory for the standard, and the observer was directed not to pay special attention to this. They are, therefore, more complex than the other errors, and less suitable for studying the relation between the magnitude of the stimulus and the error of observation. Under *V. E.*, the variable error, is given the average variation of the difference between the pulls. Thus, if in a series of ten experiments, the observer make the second pull on the average .5 kg. greater than the first, sometimes the difference will be greater than .5 kg., sometimes less, sometimes even negative; the average variation or error of these differences is the variable error, *V. E.*, in the tables. Under % *W* is given the percentage (as there are 100 experiments, it is also the number) of wrong cases; that is, those cases in which the real relation of the stimuli was the reverse of that intended by the observer. Thus, in the first quarter of the first line of figures in the table, we see that the observer, *J.*, in giving the standard pull 2 kg. =200

from memory, made it on the average 205, or $\frac{1}{40}$ too great, with an average variation in each series of 15. In attempting to make the second pull just greater than the first, the observer made it on the average 34 greater, with an average variation in each series of 17. The just noticeable difference would, therefore, be 34, or $\frac{1}{6}$ of the stimulus. This is the only figure usually recorded in experiments by this method. We, however, believe that the just noticeable difference only has an exact meaning when taken in connection with the number of errors, or the variable error. By regarding these quantities we are able to combine the several methods, and regard this as one of the chief results of our work. In the case under consideration J 's variable error was 13.1, and in the course of the 100 judgments the second pull was made less than the first three times.

SEC. 22. *Discussion of the Method and Results.*—The relation of the just noticeable difference to the magnitude of the stimulus and its variation with the two observers, and according as just greater or just less was attempted, may be conveniently seen in the accompanying curves. From the just noticeable differences

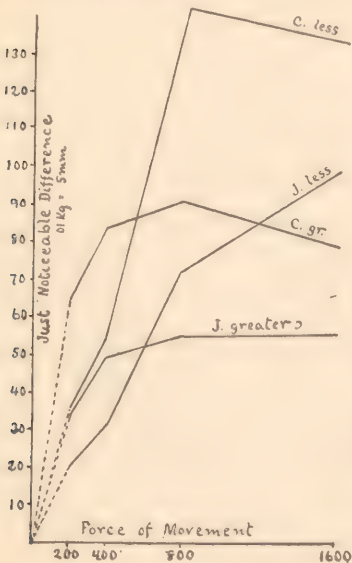


FIG. 9.

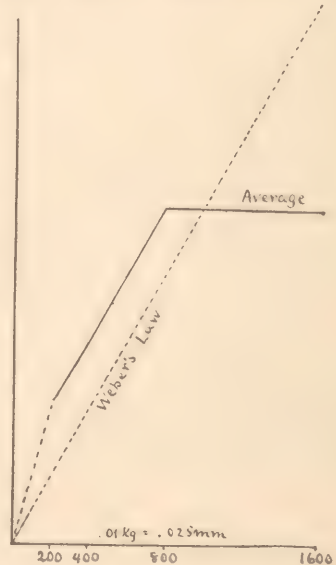


FIG. 10.

shown in the table and curves, we should be compelled to conclude that *J.*'s fineness of discrimination is nearly twice as great as *C.*'s, whereas the variable error and ratio of right to wrong cases in the same experiments give nearly the same fineness of discrimination for the two observers. In fact, *J.* was right 88% of the time, while *C.* was right 95%, which shows that the just noticeable difference meant something quite different to each of the observers. With unskilled observers we might expect still greater variation in choosing a just noticeable difference.

The just noticeable difference seems, further, to have been given a different value, according as just less or just greater was attempted. It is evident that the corresponding curves for *J.* and *C.* have much the same shape, whereas the "just less" curves differ entirely in their shape from the "just greater" curves. For the two smaller magnitudes "just less" was taken smaller than "just greater," whereas the reverse holds with the two larger magnitudes. This seems to show a marked tendency to underestimate the second pull as compared with the first in the case of the smaller magnitudes, and to overestimate it in the case of the greater magnitudes. Such a tendency is also apparent in the experiments by the method of average error (see Table X.), but is not there so marked.

If Weber's law obtained, all the curves representing the just noticeable difference in the figures would be straight lines beginning at the origin, and the average would follow the broken line. The just noticeable difference is, on the average, about one-eighth of the stimulus, but it grows rapidly smaller as the stimulus is taken greater. It is approximately

Magnitude,	2	4	8	16 kg.
Just noticeable difference,	$\frac{1}{8}$	$\frac{1}{7}$	$\frac{1}{6}$	$\frac{1}{18}$

consequently, according to the method of just noticeable difference, Weber's law does not obtain, even within narrow limits, for the force of movement.

We have given our reasons for holding that the just noticeable difference is an arbitrary point without objective criterion. We think it cannot be used as a test for comparing different observers. We see that it failed with *J.* and *C.* In Parts IV. and V. we shall find that some observers rarely think their judgment correct, unless it be in fact correct, while others are continually sure that they notice a difference which is not present. If observers be arranged according to the difference they think they can perceive, an order exactly the reverse of their real fineness of discrimination will often be obtained. An observer may, indeed, keep the just noticeable difference fairly constant under the same circumstances, but this, we think, is because he has in mind a certain apparent amount of difference or degree of confidence. When the circumstances are altered, it is very difficult for the observer to give the same value to the just noticeable difference. Thus "just greater" for *J.* was 34 with 200, and 56 with 1600. We find, therefore, that the just noticeable difference was less than doubled when the stimuli were taken eight times as great. On regarding the variable error, however, we see that for 200 it is less than half the just noticeable difference, and we conclude that the observer could usually notice this difference, and on regarding the percentage of wrong cases, we see that it was, in fact, misjudged only three times out of 100 trials. With 1600, on the other hand, the variable error is nearly as great as the just noticeable difference, and we must conclude that the observer would often fail to notice this difference; and on regarding the percentage of wrong cases, we see that it was, in fact, misjudged twenty-seven times out of 100. The just noticeable differences in the two cases do not mean the same thing, and cannot be used to measure the accuracy of discrimination; still less can they be regarded as equal mental magnitudes. The fact that the just noticeable difference tends

to increase more slowly than the variable error shows an inclination on the part of the observer to make the just noticeable difference, not an equally noticeable difference, but an equal objective difference. As in our other experiments with estimated amounts of difference, therefore, the observer tends to judge such magnitudes (doubtless through association) according to their real relations, and not according to the logarithms of these.

SEC. 23. *Reduction of the Method to that of Right and Wrong Cases.*—In making a difference so great as to be just noticeable, the observer did not aim to make it so great as to be absolutely sure of the correctness of his judgment, but so great as to feel fairly confident of its correctness, expecting in a general way to be right about nine times out of ten. If an observer could, in fact, employ such a criterion, the just noticeable difference would measure his fineness of discrimination. We should in this case, however, have substituted the method of right and wrong cases for the method of just noticeable difference. In our experiments the observer did not know the result of his judgments, and could not tell how nearly he was coming to such a standard. The table shows that, in fact, the percentages of right cases varied from 73 to 99. Still, if the probability integral may be applied to these series, we can reduce them to a common standard. From the just noticeable difference and the percentage of right cases actually obtained, we can calculate how large the just noticeable difference should have been made in order to give a certain constant percentage of right cases. We thus replace the method of the just noticeable difference by the method of right and wrong cases. The probable errors (the differences with which the observer might expect to be right 75% of the time) are given in Table IX., and the relations are shown in Figs. 11 and 12.

TABLE IX.
PROBABLE ERRORS FROM THE JUST NOTICEABLE DIFFERENCE.

	200	400	800	1600
JUST GREATER.				
J.	12.2	22.4	30.2	61.5
C.	18.3	30.1	37.3	32.4
JUST LESS.				
J.	10.5	22.	34.6	44.7
C.	16.4	29.7	58.	43.6
Av	14.3	26.	40.	45.5

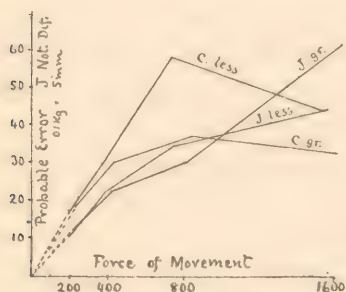


FIG. 11.



FIG. 12.

The errors given in the table and shown in the curves are evidently more regular and accordant than those for the method of just noticeable difference. They also agree with the results obtained by other methods. They show that the fineness of discrimination of the two observers is nearly the same, but that *J.* is more accurate with the three smaller, *C.* with the largest magnitude. This result seems natural, as *C.*'s arm is much stronger than *J.*'s, but it is contrary to Weber's law. As the curves for *J.* and *C.*, and it may be for "just greater" and "just

less," follow different equations, an average is fictitious, but the average curve given in the figure is evidently quite regular. It shows no inclination to follow Weber's law. The error of observation increases as the stimulus is taken greater, but much more slowly than in direct proportion to the stimulus. A curve drawn according to the square root of the magnitude is also shown in the figure. It follows fairly well the curve obtained by experiment.

The just noticeable difference converted into the method of right and wrong cases in this manner is a satisfactory test of the accuracy of discrimination. It suffers, however, from the practical drawback that a very large number of experiments must be made before a reliable result can be reached. Although one hundred judgments of each sort were made in the case before us, the value of at least one ordinate (*C.* just less 8 kg.) seems to be considerably influenced by chance. As has been explained, the method of right and wrong cases can be used most advantageously when the difference is such as to give about 84% of right cases. But this difference is smaller than most observers would call just noticeable. When the observer is always right in his decision, our theory fails us entirely, for it requires us to say that the fineness of discrimination of such an observer is infinitely greater than that of one who is right 999 times out of 1000. The theory is, indeed, saved by the assumption, that if the series were continued long enough, there would be a mistake, but we think the theory of probability cannot assign a probable time for such a mistake to occur. If several observers, even with varying differences between the stimuli, are always right in their judgments, we must regard their fineness of discrimination as alike and indefinitely great. The method of the just noticeable difference, in its original form, is thus reduced to an absurdity.

SEC. 24. *The Variable Error of the Just Noticeable Difference.*—We have seen that the method of right and wrong cases, at its best, requires many trials, whereas the variable error gives us a reliable result more quickly. In these experiments we have calculated the variable error of the just noticeable difference, and believe this to be a far better test than the just noticeable difference itself. The value of the variable error is given in the table and shown in the accompanying curves (Figs. 13 and 14).

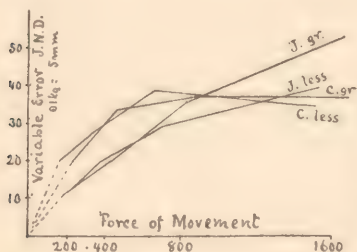


FIG. 13.

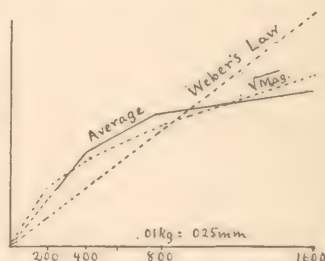


FIG. 14.

It is evident that the curves agree with those obtained independently by the method of right and wrong cases, and this proves conclusively the reliability, both of the methods and of the experiments. The average curves for the probable error and for the variable error are shown together in Fig. 16, below. The variable errors are smaller than the probable errors because they are calculated from series of 10 experiments, while the probable errors are from series of 100 experiments. The fact that the probable error of the final average is smaller for the variable error, and more especially that the variation of the separate series of the same sort is much less, shows that the variable error gives the more reliable result. We see that the accuracy of discrimination of the two observers is about the same, *J.* being the better for the three smallest magnitudes, and *C.* for the largest. The variable error for just less tends to be smaller

than for just greater, which is natural, as the magnitude aimed at is smaller. The average curve gives no indication of following Weber's law, but does tend to follow the law of the square root of the magnitude. We conclude that the variable error of the just noticeable difference is a satisfactory test of the accuracy of discrimination, but as the variable error, when just equal is attempted, is still smaller and more constant (Table X.), we see no advantage in using the just noticeable difference.

SEC. 25. *Further Results.*—We have still to call attention to the average variation of the pulls from the average of the series to which they belong, as also the absolute value of the first pull. These have turned out far more constant than we had expected. They depend on memory for the stimulus, and show that during a series of ten trials a magnitude can be held in mind very well. The average variation of the pulls is slightly larger than the variable error of the just noticeable difference, and almost exactly proportional to it, so that the curves could scarcely be distinguished. This confirms independently the results obtained by the other methods, and shows that a greater dependence on memory did not alter the relation between the error of observation and the magnitude of the stimulus.

The average variations of the several series, given in italics in the table, should be noticed, as these show what reliance may be placed on the results, they being about four times the probable error. It is, however, a somewhat complex quantity, depending not only on the normal variation of perception for the different magnitudes and methods, but also on the condition of the observer from day to day, on practice, etc.

Before leaving the comparison of methods, it may prove interesting to give curves indicating the confidence which may be placed on a few experiments. They show the results of the first ten experiments on *J.* "just greater," the general character of curves for *J.*'s "just less" and for *C.* being the same, with great

variation in the separate curves. In Fig. 16 the final results obtained by the three methods are shown together.

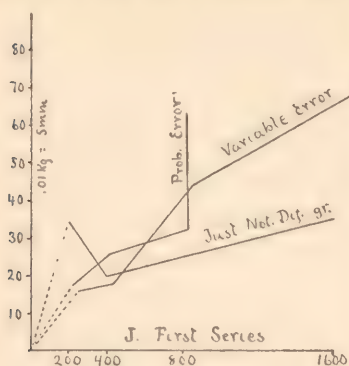


FIG. 15.

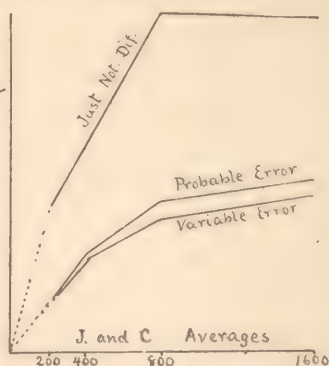


FIG. 16.

A comparison of the curves in Fig. 15 with the average of one hundred experiments will show what reliance may be placed on ten trials. Weber and others have based their laws on fewer experiments than this. From the curve for the just noticeable difference (Weber's method) the conclusion would follow that the just noticeable difference is nearly a constant magnitude whatever the stimulus may be. But the just noticeable difference here does not agree at all with the final result. It is about half the variable error and was mistaken about one-fourth of the time. That is, the observer called a difference noticeably greater that was half the time smaller than the probable error of observation. When the just noticeable difference is corrected by the proportion of right cases, an entirely different curve is obtained. It agrees very well with the final result for the first three magnitudes, but fails with 16 kg. In this case there were five errors, and the probable error must be regarded as indefinitely great. In about half the series there were no errors, and the probable error must be regarded as indefinitely small; that is, in a series of ten experiments, the method of right and wrong cases

is apt to fail. The variable error gives a curve agreeing fairly well with the final result ; but as it is nearly a straight line, it would follow from it that Weber's law obtains.

We finally conclude that the just noticeable difference cannot be used in comparing different observers, and cannot even be kept constant for the same observer under changed conditions. The method of right and wrong cases, including the application of the probability integral, gives correct results, but requires a large number of experiments (at least 100), and should be used with a difference smaller than "just noticeable." The method of average error gives the best results with a limited number of experiments (say 100), and a few (say 10) will serve for clinical and anthropometric purposes. But the average error of "just equal" is a better test than the average error of the just noticeable difference.

SEC. 26. *Experiments by the Method of Average Error.*—Experiments by this method were made by *J.* and *C.* on the same days as the experiments treated in the preceding sections. After the standard had been set by five pulls, a series of ten judgments was made, in each of which the observer first gave the standard, and then a second pull as nearly as possible equal to the first. The standard was then again set, and a second series made, in order to obtain as many experiments as by the method of the just noticeable difference. After each pair of pulls had been made, the observer was required to decide or guess which of the two had been the greater, and give the confidence of his decision. This was, naturally, nearly always *c*, little or no confidence.¹ It will be noticed that we have thus combined the two fundamental psychophysical methods.

The table of results will be readily understood, as it is much like the foregoing.

¹ *b* only occurred twice for *J.* and once for *C.* In later experiments we have subdivided *c*, so as to distinguish small confidence from none whatever.

TABLE X.
FORCE OF MOVEMENT—METHOD OF AVERAGE ERROR—J. AND C.—1600 EXPERIMENTS—IX, 1890—II, 1891.

2 KG. = 200.												4 KG. = 4.												8 KG. = 800.												8 KG. = 1600.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																				
I.			II.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.			VE			I.			V.		

In this table, two series of the same sort, each containing 100 experiments, are given for *J.* and *C.*, the final average being thus the result of 400 experiments. In so far as Pull II. is much different from I., we must regard this as due to a constant error. After the two pulls and their variations, we give the average error (*A. E.*) of the observer. If there were no constant error present, this is the only quantity we should have to consider. But, as has been explained, when there is a constant error, the average error is made up of two factors—this constant error and a variable error. Both of these factors are interesting, and deserve separate study. In a series of ten experiments, indeed, the apparent constant error is complex, being partly a true constant error, due to overestimating or underestimating the force of the second pull as compared with the first, and partly due to the variable error. In calculating the variable error of the separate series, however, we have treated the apparent constant error as a true constant error, and subtracted it algebraically from each average error to obtain an apparent variable error, *V. E.*, in the table. This was necessary, as the constant error might have been either increased or decreased by the normal variation of the variable error.

The limited number of experiments in the series makes the variable error too small, but as the series throughout contained the same number of experiments the relative value is not affected. The variation of the average error (V^2) gives an additional measure of the accuracy of discrimination.

From the table and the curves (Figs. 17 and 18) showing the relations of the variable error, we see that these are much the same as for the variable error of the just noticeable difference. The two series of the same sort on each observer correspond almost exactly, the second series showing the improvement with practice, but not altering the shape of the curves. This improvement was greater for *J.*, which is doubtless explained by

the fact that, though neither observer had previously made experiments of this sort, *C.* had had the more practice in psycho-

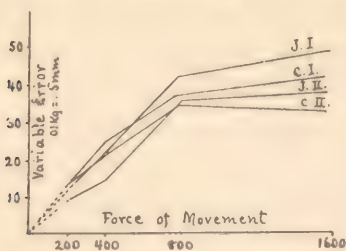


FIG. 17.

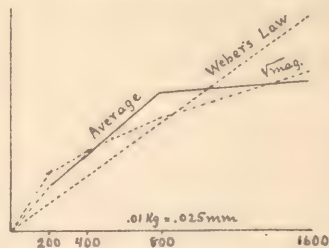


FIG. 18.

logical experiments. This calls attention to a difficulty in all anthropometric experiments. The results depend as much on adaptation to the conditions of experiment as on differences in the senses and faculties. *C.*'s error of observation was the less with the greater, and *J.*'s with the smaller magnitudes, probably due to the greater strength of *C.*'s arm. Yet this is contrary to Weber's law. It is worth notice that the experiments on *J.* throughout depart less from Weber's law than those on *C.* In the present case *J.*'s curves follow Weber's law fairly well for the three smaller magnitudes.

Curves for the constant errors of the first series are given below (Fig. 21). The constant errors show a tendency to slightly overestimate the second pull with the smaller magnitudes, and to considerably underestimate it with the greater. At first sight, one is tempted to account for the underestimation of the second pull with the large magnitudes by means of fatigue. If the observer were tired by the first pull, a second pull might seem as great when, in fact, less. But this theory would require *J.* (whose arm is the weaker) to have a larger constant error than *C.*, whereas the contrary holds, and, besides, a series of ten pairs of pulls was made continuously, and fatigue should tell if anywhere in the pairs becoming smaller, whereas the standards are overestimated.

The average or crude error, being the resultant of the constant and variable errors, is, of course, larger than the variable error, but it is no less regular. It has usually been neglected, the variable error alone being used, but it seems to us that it measures a real error of discrimination. Thus *C.*'s variable error for the larger magnitudes is smaller than *J.*'s, whereas his constant error and his average error are larger. This indicates for *C.* perhaps greater practice in such experiments, more constant attention, etc., but it is a fact that *J.* could distinguish the smaller difference. It remains a question, therefore, which of the two quantities should be regarded as the measure of fineness of discrimination.

As regards the other figures in the table, it is worth noticing that the variation of the average error is about one-half this error, while the variation of the pulls from the series to which they belong is about equal to the average error for the smaller magnitudes, but is less for the larger magnitudes, where there is a considerable constant error. Either of these quantities could thus be used to measure the error of observation, and we have in all six different quantities, any of which could be used for this purpose.

In all cases the error becomes too small for Weber's law, as the magnitude is increased, which, we believe, is the conclusion of most careful experiments on the discrimination of small differences. The values follow fairly well the law of the square root of the magnitude, but the departure is greater than can be attributed to chance variations.

SEC. 27. *Further Experiments by the Method of Average Error.*—Experiments by this method were also made with three additional observers. The results are given in the accompanying table, together with the first series made with *J.* and *C.* The variations of the series, and the variations of the average error have not been calculated, as these have been sufficiently illustrated.

TABLE XI.
FORCE OF MOVEMENT—METHOD OF AVERAGE ERROR—5 OBSERVERS AND 2000 EXPERIMENTS—IX. 1890—III. 1891.

	2 KG. = 200.				4 KG. = 400.				8 KG. = 800.				16 KG. = 1600.			
	I.	II.	A.E.	V.E.	I.	II.	A.E.	V.E.	I.	II.	A.E.	V.E.	I.	II.	A.E.	V.E.
K.	204	+ 16	32	25.2	396	+ 6	37	32.6	792	+ 17	61	55.	1580	+ 6	59	54.3
Var.	31	13	8	3.2	37	18	8	8.2	34	23	9	7.8	62	23	11	11.9
D.	189	+ 17	26	22.8	411	+ 17	45	41.2	765	+ 21	54	43.5	1625	+ 8	49	42.7
Var.	20	4	6	6.	29	12	9	9.8	29	19	8	10.7	35	22	6	5.7
F.	228	+ 18	25	18.3	408	+ 11	30	28.	794	+ 8	38	37.3	1642	— 8	44	43.4
Var.	30	8	7	5.9	47	8	4	5.8	38	10	9	7.9	60	14	10	11.
J. I.	212	+ 4	15	12.4	428	— 13	31	21.9	808	— 25	48	41.9	1630	— 17	55	48.9
Var.	7	8	4	4.2	36	17	17	7.5	32	15	14	11.1	32	19	16	14.9
C. I.	218	+ 2	15	14.4	404	— 6	27	24.2	822	— 46	59	37.	1626	— 60	68	41.8
Var.	19	6	3	2.4	20	13	7	7.7	29	29	21	8.2	31	11	9	6.
Av.	210	+ 11	23	18.6	409	+ 3	34	29.6	796	— 5	52	42.9	1621	— 14	55	46.2

The relations of the variable error to the magnitude of the stimulus for *K.*, *D.* and *F.* are shown in Fig. 19, and the averages for *K.*, *D.*, *F.*, *J. I.* and *C. I.* (together with curves for Weber's law and the square root of the magnitude) are shown in Fig. 20.

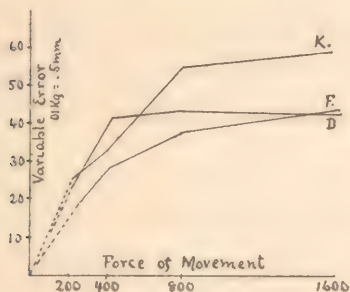


FIG. 19.

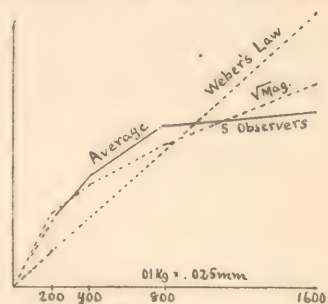


FIG. 20.

The curves for *F.* and *C.* (*C.*'s curves are given in Fig. 17) correspond closely, and this was usually the case when the two observers made the same experiments. A similarity in pursuits and interests thus seems to lead to uniformity in the processes of perception and comparison. When results correspond so well as those of *F.*, *C.* and *J.*, and the probable error for each observer is so small, we may regard the results as established, and it would not be profitable to further increase the number of experiments, unless it were in order to study the effects of practice. The curves for *K.* and *D.* bear witness to a larger error and greater irregularity, but tend toward the same shape as the others. *J.* and *K.* are women, and their curves do not permit us to draw any inference as to greater fineness of discrimination in one of the sexes.

In Fig. 20 the average curve for the several observers is given, together with a curve for Weber's law and for the square root of the magnitude. There is no doubt but that the average curve is regular and bends away from the vertical axis. Weber's law,

consequently, does not obtain. The error of observation becomes greater as the stimulus is taken greater, but more slowly. Unfortunately, we cannot compare 16 kg. with the other magnitudes, as the extent of the movement was even less than for 8 kg. But in the experiments on extent of movement (Part I.) we saw that the error was actually smaller for 700 than for 500 mm., and a similar inclination of the curve toward the horizontal axis might have occurred if movements of greater force than 16 kg. had been used.

The curve drawn according to the square root of the magnitude is evidently more like the experimental curve than is that drawn according to Weber's law, and it would correspond better if the error for 16 kg. were corrected for the smaller extent of this movement. The curves for *F.* and *C.* follow closely the theoretical curve, but the difference in the curves cannot be attributed to chance, and we may assume the presence of factors in addition to the summation of errors, for which alone the law of the square root of the magnitude accounts. Hypotheses to account for these secondary variations might easily be made, but it may be better to wait for further experiments, more particularly on magnitudes smaller than 2 and larger than 16 kg.

The combined curve for the average error would follow almost exactly the same course as that for the variable error. The average error for the individual observers, however, differs considerably from their variable error. Thus *C.* has large constant errors with 8 and 16 kg., and his average error is, consequently, much larger than his variable error; with *F.*, on the other hand, the constant error is small, and the average error is not much reduced by its elimination. *K.* and *D.* had a positive constant error with all the magnitudes, but with a comparatively large probable error. All the observers had a positive constant error with the smallest magnitude. The relation of the constant

errors to the magnitude of the stimulus can be studied most conveniently in the accompanying figures.

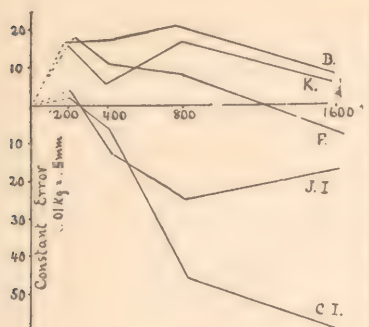


FIG. 21.

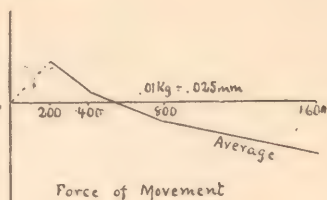


FIG. 22.

SEC. 28. *Analysis of the Error into an Error of Perception and an Error of Movement.*—In all our experiments on the perception of movement the observer first adjusted the two movements in the manner required, and afterward estimated the correctness with which this had been done. Thus, after the observer had made the force of his second movement as nearly as possible equal to that of his first, we did not consider the experiment complete, as has hitherto been the case, but required the observer to decide or guess which of the two movements had been the greater, in spite of his attempt to make them exactly alike, and to assign a degree of confidence to his decision. The degree of confidence was nearly always *c*, *a* and *b* occurring rarely except in the first few series with *K.* and *D.*

The percentages of right cases for the several observers and magnitudes are given in the accompanying table.

TABLE VII.

PERCENTAGES OF RIGHT CASES IN JUDGING THE FORCE OF TWO MOVEMENTS
MADE AS NEARLY AS POSSIBLE ALIKE.

	SECOND LESS.				SECOND GREATER.			
	2 KG.	4	8	16	2	4	8	16
K.	82	70	70	70	57	67	67	82
D.	81	92	82	91	60	55	55	57
F.	71	77	47	54	69	65	69	55
J. I. . . .	67	49	51	55	56	46	62	60
J. II. . . .	52	60	52	62	59	59	58	51
C. I. . . .	64	64	61	55	58	46	56	77
C. II. . . .	69	71	56	51	59	55	33	58

This table shows that the observers could often correctly distinguish a difference, even when they had attempted to make the two movements exactly alike. We are thus led to believe that the error which we have hitherto been considering is complex, being partly due to an error of perception and partly to an error of movement.¹ If the entire error, in the attempt to make the two movements alike, were due to an error of perception, they would seem, when completed, exactly alike, and the observer's judgment would have been a mere guess, as likely to be wrong as right. If, on the other hand, the observer did not make the two movements apparently alike, he should perceive his error of movement as a difference, and this difference would give a percentage of right cases corresponding to its size. Further, the average variable error of perception, when used as the amount of difference in the method of right and wrong

¹ This was suggested by Müller (*op. cit.* 71 sq.). He says, "Es ist aber ganz unmöglich, näher anzugeben, in welcher Weise diese beiden Factoren, die Trüchtigkeit unseres Urtheils und die Unsicherheit der Hand, das Wahrscheinlichkeitsgesetz der Einstellungsfehler bei Anwendung verschiedener Normaldistanzen beeinflussen." Our experiments, however, accomplish this.

cases, should theoretically give about 78 per cent. of right cases.¹ If the error of movement gave this same percentage, we should conclude that the error of movement and the error of perception were equal. The table shows that the percentage of right cases (apart from constant errors, which are eliminated as explained in Part IV.) was always less than 78 per cent.; we, therefore, know that in these experiments the error of movement was less than the error of perception, and by comparing the value of $\frac{\Delta}{P. E.}$ (Table I.), which would give 78 per cent. of right cases, with those values of $\frac{\Delta}{P. E.}$ corresponding to the percentages of right cases in the table, we determine the size of the error of perception as compared with the error of movement. We already know the size of the complex variable error, and can thus readily calculate the actual values of the error of perception and the error of movement, the complex variable error being equal to the square root of the sum of the squares of the separate errors.

The analysis of the variable error in judging the force of movement (Tables X. and XI.) gives the results contained in the accompanying table.

¹ So Fechner (Revision, 115), who gives the percentage as 78.4, and we used the nearest whole percentage in calculating the table. Fechner, however, seems to have made a slight mistake in his interpolation, the correct percentage being 78.7. The variable error, calculated from series of ten experiments, is, further, relatively smaller than the probable error, calculated from series of varying size. It should also be remembered that the difference varied in each experiment. But the number of experiments is too small to require great accuracy in calculation.

TABLE XIII.

ANALYSIS OF THE ERROR INTO AN ERROR OF PERCEPTION AND AN ERROR OF MOVEMENT.

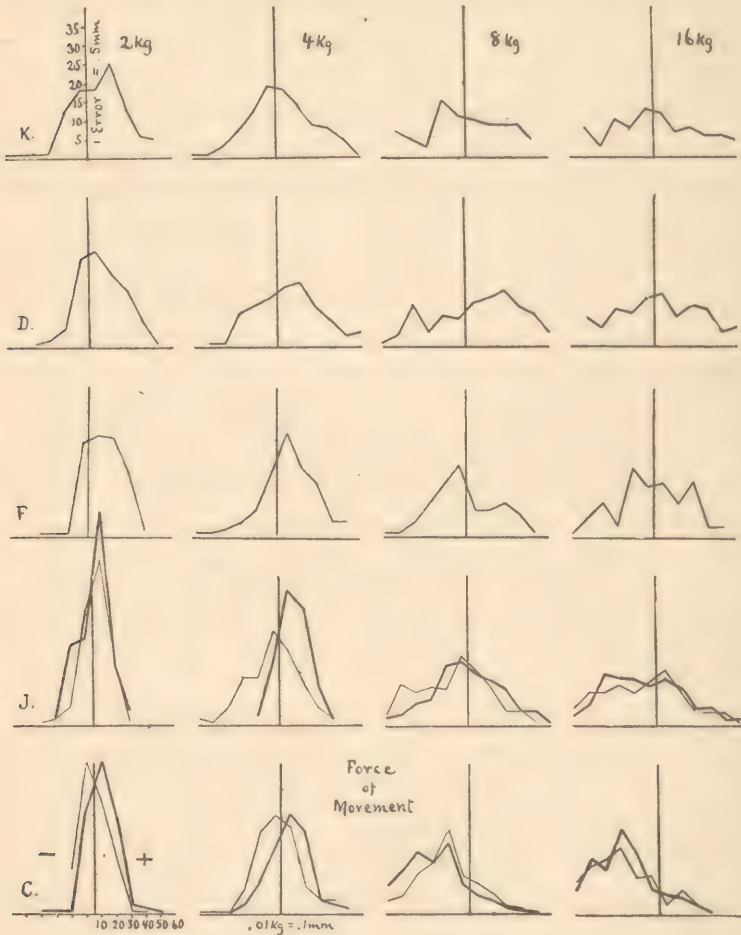
	ERROR OF PERCEPTION.				ERROR OF MOVEMENT.			
	2 KG.	4	8	16	2	4	8	16
K.	21	28	47	40	15	17	29	37
D.	18	29	40	30	13	29	24	30
F.	15	23	36	43	10	16	10	6
J. I. . . .	11	22	40	48	4	0	9	12
J. II. . . .	9	14	35	36	2	5	6	8
C. I. . . .	14	24	36	37	5	4	10	20
C. II. . . .	12	20	34	32	6	9	0	5
Av.	14	23	38	38	8	11	13	17

The averages show that the error in adjusting the movement was about half as large as the error in perceiving it, and that the variable error obtained by the method of average error is reduced by about one-eighth when the error of adjustment is eliminated. The error of perception increases more rapidly than the error of movement for the three smallest magnitudes, but is the same for 8 and 16 kg. Curves for the averages are given below (Fig. 49).

It is to be noted that the observers who had the larger complex errors had the larger errors both of perception and of movement, but that in their case the error of movement is relatively larger, being nearly equal to the error of perception. Hence we may, perhaps, conclude that exactness in movement, as needed in manual work and games, can, by training, be improved more than accuracy of discrimination. The traits latest acquired by the race and individual are the most easily altered. So far as a conclusion can be drawn from five observers, it would seem that woman as compared with man is relatively more accurate in movement than in perception.

SEC. 29. *The Distribution of Errors.*—An average cannot give as exact information as the separate quantities from which it is taken. It is like a common noun, which, for convenience, neglects individual differences. The arithmetical average together with its probable error gives, indeed, for ordinary purposes, all the information needed ; but as we have in this paper under consideration the nature of the method of average error, some study of the distribution of the errors making up the average seems desirable. In so far as this distribution follows the exponential equation (curve of error, § 3), we can discover it from the averages. But it may be useful to compare the mathematical theory with actual experiment. We have, consequently, divided the errors made by the five observers (Tables X. and XI.) into fourteen classes in accordance with their size and direction. All the errors of 20 (.2 kg.) or smaller are placed together in two classes, one for positive, the other for negative errors ; all the errors between 21 and 40 are placed in classes together, and so on up to 120, and in the seventh pair of classes all errors larger than 120 are placed. The few cases (in all 34) in which there was no error (*i. e.*, the error was smaller than the reading of the instrument) are divided equally between + and —. In place of a table, which would be somewhat cumbersome, we give curves, by means of which the distribution of errors for the several observers and magnitudes may be conveniently seen. There are 100 errors in each set, and the area of the curve may be regarded as unity. Owing to the limited number of errors, however, it is necessary to place them in classes, and a continuous curve is not, of course, obtained. The abscissæ are proportional to the size of the mean error (or mid-error) of each class, and the ordinates to the number of errors in each class. Two sets were made on *J.* and *C.*, the curve representing the second set being drawn in heavier lines.

These curves show distinctly how far the distribution of 100 errors follows the bell-shaped curve required by the theory of



FIGS. 23 TO 42.

probability. All the curves do, in fact, tend to approach the theoretical curve, and the agreement is best for the best observers (*F*, *J*, and *C*), and for the stimulus (4 kg.) in which the

relation of the ordinates to the abscissæ is the most favorable for drawing the curve. If the number of errors were greater than 100, the curves would of course become more regular. For various reasons we are not prepared to introduce into this paper more mathematical discussion than necessary. But these curves might be treated in a manner which would compare the average and variable error, and the relation and relative accuracy of these compared with the probable error, the error of mean square and the modulus. The departure from the theoretical curve could further be used to discuss and possibly to amend the assumptions made by Laplace and Gauss in their mathematical deductions.

Many of the facts already discussed may be observed with advantage in these curves. Thus the taller and narrower curves show distinctly which of the observers were the more accurate, and which of the stimuli were the more exactly perceived. The position of the curve to the right or left of the perpendicular axis shows the amount and direction of constant errors. The two curves given for *J.* and *C.* show the accuracy with which the same series can be repeated and the improvement with practice.

SEC. 30. *The Summation of Errors.*—Our theory to account for the increase of the error of observation with the size of the magnitude assumes that the algebraic sum of a number of errors increases as the square root of the number. It has seemed to us that it might be worth while to test the mathematical theory by experiment, as we are not aware of this having been directly done. We have therefore added in pairs algebraically the variable errors (with 2 and 4 kg.) for *F.*, *J.* and *C.* As there were ten errors in each series we thus obtained five complex errors, and took the average of these. According to theory, this average should be nearly the variable error, multiplied by

$\sqrt{2} = 1.414$, and should further be equal to the variable error made with a stimulus double the size. In the accompanying table we give the variable error (*V. E.*), its value when multiplied by the $\sqrt{2}$, the result of adding the errors algebraically in pairs (*S.E.*), which should theoretically be nearly the same as the preceding, and the variable error with a stimulus double the former, which should be the same as the two preceding, supposing the error of observation to be due entirely to the summation of errors.

TABLE XIV.
SUMMATION OF ERRORS—1000 EXPERIMENTS.

	2 KG. = 200.				4 KG. = 400.			
	V.E.	$\times \sqrt{2}$	S.E.	V.E. 4 KG.	V.E.	$\times \sqrt{2}$	S.E.	V.E. 8 KG.
F. . . .	18.3	25.9	23.4	28.	28.	39.6	34.1	37.3
J. I. . .	12.4	17.5	15.8	21.9	21.9	31.0	33.3	41.9
J. II. . .	9.5	13.4	12.6	14.4	14.4	20.4	18.7	35.6
C. I. . .	14.4	20.4	20.1	24.5	24.5	34.6	34.4	37.
C. II. . .	13.1	18.5	17.	21.4	21.4	30.3	35.5	34.1
Av. . .	13.5	19.1	17.8	22.	22.	31.2	31.2	37.2

From the figures given in the table we see that the summation of errors corresponds closely to the variable error multiplied by $\sqrt{2}$. In eight sets out of ten it is, however, slightly smaller and in a tolerably constant ratio. At all events the mathematical theory, according to which combined errors increase as the square root of the number of observations, and the probable error of an average decreases as the square root of the number, is confirmed fairly well by these experiments.

The error of observation when the stimulus was doubled was greater than either mathematical theory or actual summation of the errors admits. We consequently assume that the error of

observation was increased by the summation of errors in the manner required by theory, but also by other factors entering into the execution of the movement. It would not be likely that the error made (say) in giving 2 kg. would be the same as in going on from 2 to 4 kg. Many factors—the magnitudes most commonly compared in daily life, the muscular mechanism used, the strength of the observer, fatigue, etc.—would alter the error of observation with different magnitudes, and these must be empirically determined by experiment. But while these would sometimes increase and sometimes decrease the error of observation, the general tendency of the error to increase as the magnitude is taken larger is accounted for in a satisfactory manner by the summation of errors.

SEC. 31. *Experiments by the Method of Estimated Amount of Difference.*—This method was used by *J.* and *C.* in conjunction with the experiments just described. The attempt was made to double 3 kg., to halve 12 kg., and to find a mean between 2 and 4 kg., and 4 and 8 kg. respectively. The standards were set in the manner already described. When the method of mean gradation was used, two standards had to be determined and held in mind; after this had been done in five trials the observer gave one pull (the greater or less in alternate series), then the second, and thirdly a pull which seemed midway between the two. The error of each experiment was separately calculated. In halving 12 kg. the dynamometer was set at 4 kg., the extent of this pull being consequently proportional to the force of 8 kg., when the stop was set at zero. The magnitudes to be estimated were chosen so as not to be the same as any of the standards. The results of these experiments are given in the accompanying table.

TABLE XV.

FORCE OF MOVEMENT—METHOD OF ESTIMATED AMOUNT OF DIFFERENCE—J. AND C.—800 EXPERIMENTS—IX. 1890—I. 1891.

	DOUBLE 3 KG. = 300.								HALVE 12 KG. = 1200.							
	I.	V.	II.	V ¹ .	A.E.	V ² .	V.E.		I.	V.	II.	V ¹ .	A.E.	V ² .	V.E.	
J. . .	326 14	26 6	— 10 92	48 14	72 17	37 6	56 8		1238 33	43 10	+ 266 50	41 9	266 50	41 11	43 14	
C. . .	312 24	24 6	— 131 39	37 8	132 39	44 12	46 10		1232 38	53 17	+ 239 49	37 11	239 49	34 6	34 4	
Av. .	319	25	— 70	42	102	40	51		1235	48	+252	39	252	37	38	

	MEAN BETWEEN 2 AND 4 KG.										MEAN BETWEEN 4 AND 8 KG.									
	I.	V.	II.	V ¹ .	III.	V ² .	A.E.	V ² .	V.E.		I.	V.	II.	V ¹ .	III.	V ² .	A.E.	V ² .	V.E.	
J. . .	231 11	20 6	428 28	28 7	— 7 21	27 7	36 13	18 6	28 10		424 34	31 9	849 33	44 14	— 44 40	39 9	69 21	32 12	44 16	
C. . .	236 24	19 7	417 32	31 4	— 3 38	26 6	41 11	18 6	24 9		450 36	38 10	858 18	51 13	— 54 17	48 16	63 16	33 12	42 8	
Av. .	233	19	422	29	— 5	26	38	18	26		437	34	853	47	+49	43	66	32	43	

In attempting to double a given pull *J.* did so very nearly, whereas *C.* gave a pull scarcely more than half again as great as his first pull. We cannot explain this difference in the two observers, whose variable error here as elsewhere agrees very well, but consider it of interest as showing how difficult it is to make quantitative estimates of sensations. We, ourselves, think that such estimates depend on association with known quantitative relations of the stimuli, and it seems quite likely that these should vary greatly with different observers. This

experiment of doubling a stimulus (sensation?) would seem to reduce Fechner's law to an absurdity. If we measure the intensity of a sensation by estimating the number of just noticeable differences which it contains, and if the just noticeable difference be proportional to the magnitude of the stimulus, the difference between no sensation and the force of 3 kg. ought to seem immensely greater than the difference between 3 and 6 kg., and in attempting to double 3 kg. the observer ought to give a pull beyond the strength of any arm. But, in fact, both observers less than doubled the first magnitude.

In attempting to halve 12 kg., the two observers agree tolerably well in their estimates, greatly overestimating the true half. This, too, is exactly contrary to the result required by Fechner's law. The experiment was complicated by the fact that the dynamometer was set at 4 kg., so that both observers more nearly halved the extent than the force of the movement. This indicates that throughout these experiments the observer may have based his judgment largely on the extent (and possibly the time) of the movement.

The results of the two observers in using the method of mean gradation agree very well. The arithmetical mean is underestimated, and so far this is the result required by Fechner's law. But the estimates do not give the geometric mean.

The results of experiments by this method are considerably influenced by contrast, as has been found to be the case with lights.¹ Thus when the order was 200—400—estimated mean, J 's constant error was —28, and C 's —50; whereas, when the order was 400—200—estimated mean, J 's constant error was +14, and C 's —34. With 4 and 8 kg., a similar but less marked result was

¹ Alfr. Lehmann, Ueber die Anwendung der Methode der mittleren Abstufungen auf den Lichtsinn, *Philos. Stud.*, III. 497, 1886.

Hjohmar Neiglick, Zur Psychophysik des Lichtsinns, *Philos. Stud.*, IV. 28, 1887.

obtained for both observers. These, like many other effects of contrast, seem somewhat difficult to explain. We may, if we like, assume that the latter standard was somewhat better in mind than the former, and a greater objective difference between it and the judgment-pull seemed equal to a less difference between this judgment-pull and the more vaguely remembered first standard.

The variable error in these experiments is more nearly alike for the two observers, and has a smaller probable error than the constant error of estimation, and is better suited for measuring the fineness of discrimination. But for ordinary purposes it cannot be recommended, as the variable error, when "equal" is attempted, is less complex and more regular.

It will be remembered that experiments on extent of movement by this method also gave an irregular constant error, but that the half of a movement was also overestimated and the double underestimated. In that case, the mean tended to be overestimated. All our experiments, therefore, are contrary to the assumption that sensations are estimated as proportional to the logarithm of the stimulus. We believe that differences (they may be qualitative) in sensation, being associated with quantitative differences in the stimuli, are used to estimate the relations of these magnitudes, and our experiments show that such estimations are subject to large constant errors, but tend to discover the true objective relations of the extent and force of movements.

PART III.

ON THE TIME OF MOVEMENT.

SEC. 32. *Introductory*.—Experiments on the time of movement have not, we think, hitherto been published. Numerous experiments have, indeed, been made on the estimation of intervals of time and on the accuracy with which a rhythm can be kept.¹ These researches contradict each other concerning the validity of Weber's law, and in most other respects. Galton has recently recommended the rate of movement as an anthropometric test, and devised an instrument for its measurement.²

Our chief object was to determine the accuracy with which the time of movements can be discriminated, and to study the part played by rate as compared with extent and force in the perception of movement. We also give some experiments on the maximum rate of movement, which subject deserves study, if only on account of its possible pedagogical and clinical applications.

SEC. 33. *Apparatus and Methods*.—A detailed description of the apparatus needed to measure short intervals of time with exactness need not be given here. The arrangement which we consider the most convenient has been described elsewhere by

¹ The most recent, by Herbert Nichols, "The Psychology of Time," *Am. Jour. of Psy.*, Vol. III. 4, IV. 1, 1891, who gives an excellent summary and criticism of preceding researches, twenty-one in number.

² Francis Galton, *A New Instrument for Measuring the Rate of Movement of the Various Limbs*, *Journal of the Anthropological Institute*, 1890.

one of us.¹ It consists of an electric chronoscope, with the pieces necessary to control and regulate it. The great accuracy of this apparatus is sufficiently shown by the fact that the average variation in the time of fifty blows (in five series) in one case was 1.6 thousandth of a second, which very small variation includes the error of all the apparatus as well as the irregularity of the blow. In addition to the apparatus for measuring the time, we needed an instrument which would close an electric current when the movement was begun, and break it when the movement was ended. This is shown in Fig. 43.



FIG. 43.

An electric current passes through the contacts at C and C' . The observer sat in a convenient position in front of the instrument, and placed his right hand on H and against the bar AHC . When the hand was moved the spring drew up the bar and closed the contact. We give a special figure of this contact. We should like to call attention to it, as we think it might be useful in physical instruments. Contacts are usually closed by means of mercury, but owing to combustion and spilling it is

¹ Mind, XI. 1886; Phil. Stud., III., 1886. We have, however, introduced several improvements. The chronoscope, as made by Peyer and Favarger, often fails to give the proper time. We have obviated this fault by replacing the string with which the instrument is started by a bar and key, by which means it can be started constantly and regularly, and by placing a clamp on the carriage of the tuning-fork, which prevents it from becoming loosened with the vibrations. A wooden house, with padding placed over the clock-work, deadens the noise and keeps out dust. We also constructed a new regulator, made by C. Krille, Leipzig.

difficult to keep the mercury at a constant level. Besides, mercury is always troublesome in a laboratory. This contact is closed at an exact point by the platinum bar touching the platinum sunk into the inclined plane at *C*. The pressure can be regulated by the spring and screw at *S*, and the contact is kept clean by the rubbing. The contact is never broken after it has once been made. There is no rebound, as the movement is stopped gradually by the increasing friction of the inclined plane.

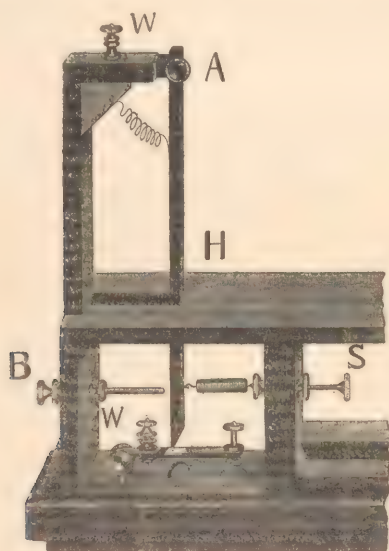


FIG. 44.

When the hand has moved from *H* to *R* and touches the bar, the lever breaks the contact at *C'*. This bar is very light, and the most violent blow will not injure hand or apparatus. The electric current is thus closed while the hand moves, and the time of the movement in thousandths of a second is directly read from the chronoscope. The duration of either a quick blow or a slow movement can thus be measured conveniently and exactly.

In our experiments on the discrimination of the time of movement, the extent was always 50 cm. The force of the blow therefore varied, being greater for the quicker movements. The observer could, consequently, use the perception of a difference in force to judge the time. We regret that we have not been able to carry out experiments which we had planned to discover how the judgment of the time of movement is affected by alterations in the extent and force. The time between the two movements was made as nearly as possible one second, but owing to the nature of the experiments this interval could not be given to the observer.

We used as normal times 1, $\frac{1}{2}$ and $\frac{1}{4}$ sec., and the time of a blow made as quickly as possible. This blow required about $\frac{1}{8}$ sec., so we had a geometric series of four magnitudes, as with the force of movement. The normal time was given to the observer in the same manner as in the experiments already described. He made a movement in what he supposed to be about (say) 1 sec., and was told the direction and amount of his error. After he had learned the approximate size of the standard by five such trials, a series of ten judgments was made. The observer made the normal movement, and then a second movement, as nearly as possible, in the same time. The four magnitudes were used at one sitting, the order being varied, so as to eliminate the effects of contrast and fatigue.

SEC. 34. *Results of Experiments.*—By the method of average error 400 experiments each were made on *K.*, *D.*, *F.* and *C.* The method of calculating and tabulating the results was the same as in the corresponding experiments on extent and force. The table will, therefore, be readily understood.

TABLE XVI.
TIME OF MOVEMENT—METHOD OF AVERAGE ERROR—4 OBSERVERS—1000 EXPERIMENTS—XI. 1890—III. 1891.

	MAXIMUM.				$\frac{1}{4}$ SEC. = 250 σ .				$\frac{1}{2}$ SEC. = 500 σ .				1 SEC. = 1000 σ .				
	I.	II.	A.E.	V.E.	I.	II.	A.E.	V.E.	I.	II.	A.E.	V.E.	I.	II.	A.E.	V.E.	
K..	188	— 13	26	23.7	283	+	8	37	31.3	496	+	11	61	87.8	+	45	133
Var..	35	13	15	12.9	10	16	6	5.7	38	13	21	19.3	115	65	24	27.6	
D..	136	— 16	21	14.2	215	—	4	38	36.7	544	+	16	96	1020	+	41	168
Var..	9	10	9	3.8	21	10	10	11.7	71	34	28	26.3	99	86	43	32.7	
C..	87	— 5	12	10.	286	+	3	28	24.7	480	+	27	47	1055	+	36	78
Var..	6	6	4	2.4	13	9	5	5.1	31	19	8	7.9	109	22	15	12.9	
F..	110	— 7	13	9.7	273	— 14	24	18.8	492	+	2	35	34.1	959	+	69	88
Var..	6	6	5	4.3	14	6	6	6	34	24	7	8.7	87	31	30	15.6	
Av.	130	— 10	18	14.4	264	— 2	32	27.9	503	+	14	60	52.0	978	+	48	99.8

We have in the table the time of the first movement (I.), the time of the second movement (II.) compared to the first (the constant error), the average error (A. E.) in attempting to make the two movements alike, and the variable error (V. E.) obtained by eliminating as far as possible the constant error from the average error. One-thousandth of a second (σ) is used as the unit of time.

The variable error has usually been made the measure of discrimination in experiments by the method of average error. We give in Figs. 45 and 46 curves showing the relation of the variable error to the magnitude of the stimulus. The relations given

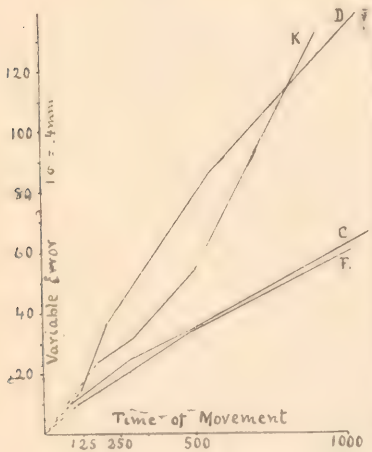


FIG. 45.

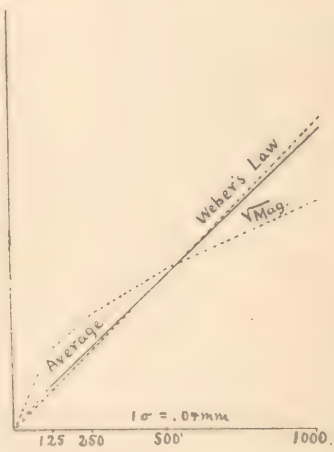


FIG. 46.

in the table and shown in the figures prove that the error in judging the time of movement tended to vary as the time, although, as is usually the case, the error becomes too small as the magnitude is increased. The apparent close agreement of the average with Weber's law is, indeed, due to the irregularity of K's curve. The curves for the other observers depart about

equally from Weber's law and from the square root of the magnitude.¹

SEC. 35. *The Constant Error.*—The average error is, of course, larger than the variable error, but would give nearly the same curves. It is the constant error which has received most attention in experiments on rhythm and the sense of time, but different investigations differ greatly in their results. We give curves showing the constant errors in our experiments (Figs. 47

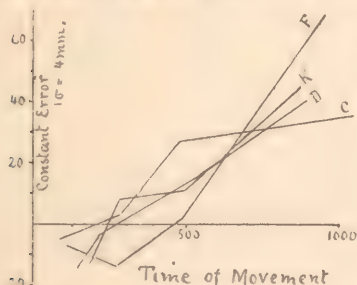


FIG. 47.

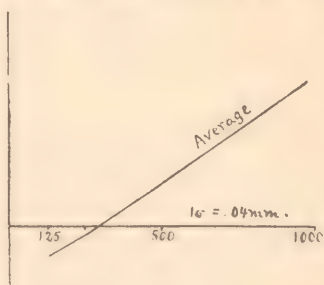


FIG. 48.

and 48). The curves for the several observers agree better than is usually the case with constant errors. The average curve is nearly a straight line crossing the horizontal axis just beyond one-fourth second. With one-half and one second, the second movement was made more slowly than the first. With the maximum blow the second movement was made more quickly than the first and also seemed easier and quicker. The accomplishment of one blow thus makes one following it quicker and more powerful.

The error made by an observer in judging a standard magnitude is usually very great. Before making these experiments the observers were required to estimate one, one-half and one-fourth second. The averages of ten such estimations are given in the accompanying table.

¹ One of the writers (J. McK. C.) thinks that the comparatively large errors with the slower movements were due to the fact that the force of the blow was used in judging the quicker movements—force being judged more accurately than time.

TABLE XVII.

ESTIMATION OF A STANDARD INTERVAL OF TIME—4 OBSERVERS—100 EXPERIMENTS

	$\frac{1}{4}$ SEC. = 250 σ .		$\frac{1}{2}$ SEC. = 500 σ .		1 SEC. = 1000 σ .	
	TIME.	V.	TIME.	V.	TIME.	V.
K.	..	..	..	..	201	10
D.	171	15	143	6	274	20
C.	390	63	616	115	901	226
F.	191	19	273	27	437	55
Av.	276	32	344	49	453	78

The table shows that one second was much underestimated, although all the observers had been using one second (given by a pendulum) as the time of movement in other experiments. The attempt to estimate longer intervals of time and other standard magnitudes gave very great errors, which shows how little reliance should be placed on such estimates—for example, in a court of justice.

As we did not make other experiments on the time of movement by the method of estimated amount of difference, it is worth noting that in the present case when the observer attempted to give an interval half as great as another (the order of the series was one, one-half, and one-quarter second), the half was greatly overestimated. All through the experiments the same constant error was present: one-half second seemed to *K.* and *C.* less than the half of one second, and more than the double of one-fourth second. That is, the estimation of intervals of time gives a result contrary to Fechner's law. This increase in subjective estimation of equal objective increments may be related to the fact that the error of observation increases faster than the objective increment warrants, according to the summation of errors. Both facts are perhaps due to the

greater part played by force in the quicker movements, and may be related to the commonly observed phenomenon that time passes more quickly when we are active than when we are passive.

SEC. 36. *Analysis of the Error into an Error of Perception and an Error of Movement.*—As in the corresponding experiments on extent and force, the observer, after he had made the time of the two movements as nearly as possible alike, was required to judge the success with which this had been done. The percentages of right cases for the several observers and magnitudes are given in the table.

TABLE XVIII.

PERCENTAGES OF RIGHT CASES IN JUDGING THE TIME OF TWO MOVEMENTS MADE AS NEARLY AS POSSIBLE ALIKE.

	SECOND LESS.				SECOND GREATER.			
	MAX.	250.	500.	1000.	MAX.	250.	500.	1000.
K.	100	88	83	69	5	40	51	69
D.	82	76	67	47	63	64	79	82
F.	82	68	86	83	32	75	51	61
C.	67	58	57	73	20	76	62	58

The table shows that the movements could be judged much better than they could be made. The great excess of right cases with the maximum blow, when the second movement was the quicker, shows that the second blow seemed far quicker and more powerful than the first. It was, in fact, quicker, but less so than it seemed.

When the complex variable error given in Table XVI. is analyzed into an error of perception and an error of movement in the manner described (Sec. 28), we obtain the results given in Table XIX., and the accompanying curves (Fig. 50).

TABLE XIX.

ANALYSIS OF THE ERROR INTO AN ERROR OF PERCEPTION AND AN ERROR OF MOVEMENT.

	ERROR OF PERCEPTION.				ERROR OF MOVEMENT.			
	MAX.	250.	500.	1000.	MAX.	250.	500.	1000.
K.	22	26	46	112	10	16	29	72
D.	11	30	66	122	9	21	53	66
F.	9	15	28	47	3	11	20	38
C.	10	21	33	58	0	12	10	31
Av.	13	23	43	85	5	15	28	52

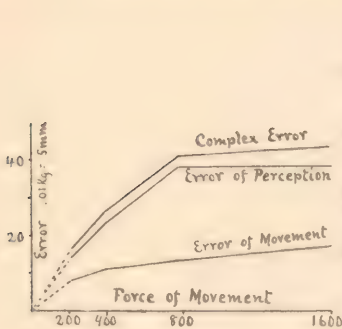


FIG. 49.

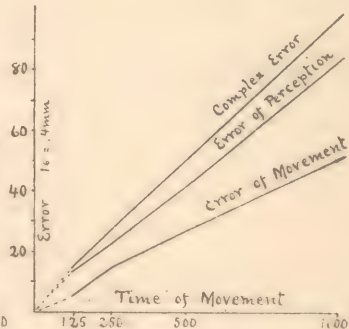


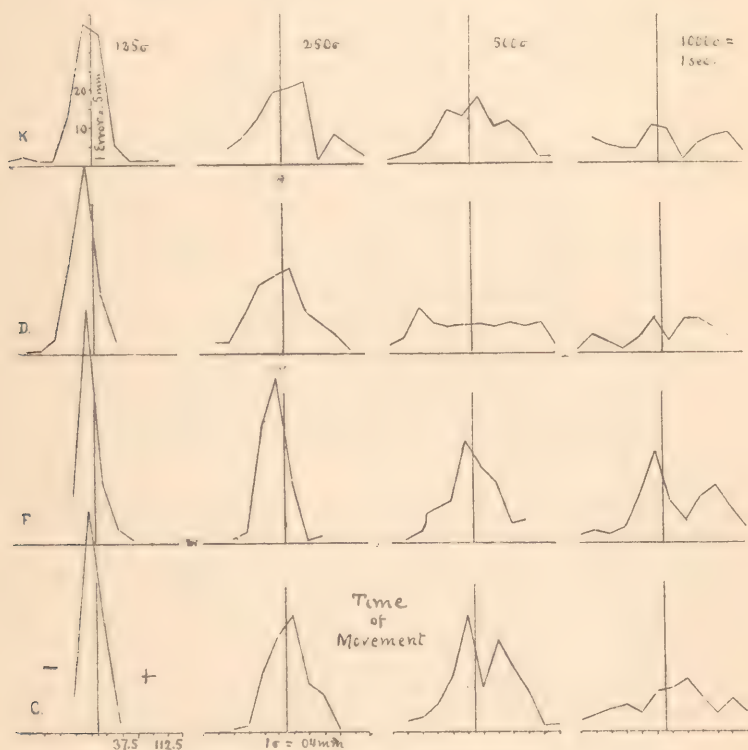
FIG. 50.

We thus see that both the error of perception and the error of movement increase nearly in direct proportion to the magnitude of the stimulus, but somewhat more slowly. The error of perception is somewhat less than double the error of movement, and about four-fifths of the complex variable error.

In Fig. 49 the corresponding curves for the force of movement are shown. In comparing the curves it must be noticed that the ordinates representing the error in force are twenty times the abscissæ, whereas those representing the error in time are only ten times the abscissæ. It is evident that force was

judged relatively much better than time, especially for the larger magnitudes.

SEC. 37. *The Distribution of Error.*—The errors in adjusting the time of movements were classified in the same manner as those on the force of movement (Sec. 29). The accompanying curves show clearly the distribution of small and large errors, and the variation for the several observers and magnitudes. The classes are 0–25 σ , 26–50 σ , etc.



FIGS. 51-66.

The curves are tolerably regular and approach the bell-shape required by the theory of probability. Small errors are evi-

dently more frequent than large ones. The taller and narrower curves of F . and C . bear witness to greater fineness of discrimination, and their more regular shape to smaller variation. Regularity cannot be expected with the larger magnitudes, as there were comparatively few small errors.

SEC. 38. *The Summation of Errors.*—In exactly the same manner as in the experiments on the force of movement (Sec. 30), we added algebraically in pairs the variable errors of F . and C . with $\frac{1}{4}$ and $\frac{1}{2}$ second. The results are given in the table.

TABLE XX.
SUMMATION OF ERRORS—400 EXPERIMENTS.

	$\frac{1}{4}$ SEC. = 250 σ .				$\frac{1}{2}$ SEC. = 500 σ .			
	V.E.	$\times \sqrt{2}$.	S.E.	V.E. $\frac{1}{2}$.	V.E.	$\times \sqrt{2}$.	S.E.	V.E. I.
F. . . .	18.8	26.6	29.2	34.1	34.1	48.2	50.3	60.6
C. . . .	24.7	34.9	35.4	35.1	35.1	49.6	49.2	66.7
Av. . . .	21.7	30.7	32.3	34.6	34.6	48.9	49.7	63.6

It is evident that the results obtained by the summation of the errors correspond very closely with the variable error multiplied by the square root of two. The variation is not larger than the probable error would lead us to expect, and the mathematical theory is thus confirmed by the experiments. The variable error in these experiments on the time of movement increases, as we have seen, more rapidly than the mere summation of errors would warrant.

SEC. 39. *The Maximum Rate of Movement.*—We believe that the time of the quickest blow which can be made is a useful test for clinical and anthropometric purposes. Under constant conditions it is very regular, but differs considerably with different observers and under changed conditions. In ten series of ten

experiments made on *C.* the average variation of a blow fifty cm. in extent was only 5σ . The average time of the separate series, however, varied from 96 to 138σ , and this variation tells of some change in the condition of the observer. In certain diseases of the nervous system the rate of movement might be most useful as a test of progression or recovery, perhaps fully as valuable as the dynamometer test now used universally. The difference in the rate of movement with right and left hand (or foot) may also prove useful in the diagnosis of unilateral disease. The rate of movement for *C.* as the result of 120 experiments was exactly the same (119σ) for the two hands. *C.*'s reaction time with right and left hand is also the same, and this test may also be recommended for clinical experiment. For *C.*, a blow with the right hand from right to left was a little slower (7σ as the result of 120 experiments) than from left to right.

In the experiments which we have described (Table XVI.) the maximum rate of movement for the four observers was respectively 87, 110, 136 and 188σ , and the results obtained from other observers fell within these limits. The time for women is decidedly longer than for men, which measures a well-known difference in the sexes, due doubtless to selection and heredity.

In these experiments only the time of the entire movement was measured, its extent being always 50 cm. We also made a few experiments in which the extent was varied, in order to study the rate of a movement during its course. As the result of 200 experiments on *C.* we obtained the following times:

Extent,	10	30	50	70 cm.
Time,	13	63	104	141 σ

The motion was therefore nearly uniform at the rate of 5 M. per second. But at the beginning of the movement there was apparently a greater followed by a lesser rate.

PART IV.

ON LIFTED WEIGHTS.

SEC. 40. *Introductory*.—Weber's generalization concerning the relation of the least observable difference in sensation to the magnitude of the stimulus was chiefly based on experiments with lifted weights.¹ These experiments are of considerable historical interest, but they were too few and too inexact to contribute to our present knowledge. Fechner's research with lifted weights is well known, and has, perhaps, never been surpassed in extent and thoroughness by any investigation in any science. The experiments were made 1856-1859, and Fechner always intended to publish the details, but left this unaccomplished. Summaries are, however, contained in the *Elemente* and in the *Revision*. Fechner made 67072 experiments on himself by the method of right and wrong cases. The normal weight was varied between 300 and 3000 grammes, and the "comparison weight" was from 1.5 to 8 per cent. heavier. The experiments show that the percentage of right cases ($+ \frac{1}{2}$ the doubtful cases) remained tolerably constant, when the difference between the two weights was proportional to the normal weight. The experiments thus tend to support Weber's law. The percentage of right cases, however, generally becomes less, as the normal weight is taken smaller, and this Fechner explains by supposing that the weight of the arm should be added to both of the weights. The most serious criticism to which Fechner's experiments are open

¹ See references, Sec. 2.

would seem to be that it is difficult, when the relations of the weights are known, to compare the sensations without taking the known differences of the stimuli into account. Fechner defends¹ this method, naïvely arguing that its objectivity is proved by the close correspondence of his experiments with the law in defence of which they were undertaken. We had intended to compare the results of experiments, when the subject knew the objective relations of the stimuli, with those in which he was ignorant of these. We have, however, only found that it is more difficult for us to come to a decision when we know the objective relations. The large number of doubtful cases recorded by Fechner (they are nearly one-seventh of the whole number) seems, indeed, to bear witness to a similar difficulty.

Hering² gives an account of some experiments on lifted weights made by Biedermann and Loewit at his suggestion. These seem to show that the least noticeable difference becomes greater as the stimulus is taken greater, but much more slowly than in direct proportion. The variation from Weber's law is thus in the same direction as in Fechner's experiments, but far greater.

Merkel,³ in his extended psychophysical study, found Weber's law to hold for pressure made by the moving finger with great exactness, the least perceptible difference being $\frac{1}{15}$ of the stimulus. These experiments were made by Merkel on himself, and we judge them to be in need of independent confirmation. Pierce and Jastrow (*op. cit.*) found the probable error for pressure, from which the muscular sense was not entirely excluded, to be about $\frac{1}{20}$ of the stimulus (250 gm.), and to be further diminished by practice.

¹ Revision, p. 58.

² Ueber Fechner's psychophysisches Gesetz, Sitzungsber. d. Wiener Akad. d. Wiss., III. Abth., 1875, p. 33.

³ *Op. cit.*

Several observers have studied the perception of lifted weights in order to determine what part is due to the skin sensations, and what part to the muscular sensations. These experiments have been made on paralytic patients, and by stimulating the muscle electrically.¹ They seem to show that the discrimination of lifted weights is not interfered with, though the skin be ataxic, or though the movement be caused by electric stimulation. But, as is so common in clinical experiments, cases are cited from which exactly opposite conclusions are drawn.

The only research remaining for us to mention is the recent interesting paper by Müller and Schumann, already noted. In view of the effects of contrast, etc., they study the nature of the motor impulse, and conclude that the velocity with which a weight is lifted is the chief factor in the discrimination of the force of movement.

Our experiments with lifted weights were made with a view to studying methods for determining the fineness of discrimination and the factors entering into the perception of movement. They are not concerned with the relation between the magnitude of the stimulus and the error of observation, and were mostly made earlier than the experiments in the foregoing Parts of this paper.

SEC. 41. *Apparatus and Methods.*—When lifted weights are used as stimuli they can be easily arranged and accurately measured. We loaded wooden boxes so that they weighed (to 100000) 100, 104, 108, 112 and 116 gm. Care was taken to place the load in the centre of the box and to make the packing firm. The boxes were cylinders about 6 cm. in diameter and 3 cm. high. The weights looked alike, and several sets were used, so that the observer might not by any chance learn them. In the later experiments paper caps were made, and

¹ References will be found in Wundt, James and Müller.

shifted from weight to weight. But these precautions were scarcely necessary, as the observer did not look at the weights. The observer sat on an ordinary chair facing a low table. The boxes were placed, two at a time, in a line parallel to the edge of the table, but a little back and opposite the left shoulder. A seconds pendulum swung in front of the observer.

The recorder placed the 100 gm. weight and one of the heavier weights for the observer. One was about 1 cm. to the right of the other, and this was always lifted first. The weight was lightly grasped on the side with the thumb and fingers, the observer being allowed to choose the position which seemed to him the most convenient. The weights were not raised perpendicularly, but carried along from left to right. We chose this movement because we thought it could be kept the most constant. The movements were made in time with the pendulum, and in the direction in which it swung. The first weight was lifted, as nearly as possible, for one second, during the next second the position was recovered, and, lastly, the second weight was lifted for one second. The time was easily kept, as the motion of the pendulum was followed almost automatically. It was our intention to have the extent of movement 30 cm., but all the observers showed a tendency to make it greater, and we did not check this. A swing of the arm of about 40 cm. would thus seem to be most natural, as there is a most natural swing of the leg in walking, due to its acting as a pendulum.

After the observer had lifted the two weights he was required to say whether the second weight seemed lighter or heavier than the first. He was also required to estimate what confidence he felt in his decision. a , b and c were used in the manner already described, a meaning sure of the correctness of the decision, b less certain, and c uncertain. If the observer could discern no

difference whatever, he was allowed to say "doubtful" (*d*), but was asked to use this as seldom as possible, and in our later experiments he was required to guess even when quite doubtful. Forty experiments were made in a series, each of the four differences being used ten times, and the second weight being, in half the cases, the heavier, in half the lighter. The order was irregular, and could not have been guessed by the observer. Ten series were made with each observer, so that 100 trials were made with each difference. The observer was always ignorant of the real relations of the weights, and only the writers knew the results of previous experiments.

SEC. 42. *Results of Experiments.*—The results of 4000 experiments made on nine observers are contained in the accompanying table. The number of right, of wrong and of doubtful cases is given, for each amount of difference, and according to whether the second weight was the lighter or the heavier.

TABLE XXI.

THE DISCRIMINATION OF LIFTED WEIGHTS WEIGHING ABOUT 100 GM.—METHOD OF RIGHT AND WRONG CASES—9 OBSERVERS—
4000 EXPERIMENTS—II. 1890—I. 1891.

100:	SECOND WEIGHT THE LIGHTER.						SECOND WEIGHT THE HEAVIER.					
	104.		108.		112.		104.		108.		112.	
	R.	W. D.	R.	W. D.	R.	W. D.	R.	W. D.	R.	W. D.	R.	W. D.
R.	36	14 0	35	15 0	45	5 0	28	22 0	36	14 0	41	9 0
K.	25	25 0	30	20 0	40	10 0	40	10 0	43	7 0	46	4 0
G.	17	27 6	29	15 6	32	13 5	43	7 0	47	1 2	48	1 1
L.	33	17 0	36	14 0	45	5 0	37	13 0	39	11 0	49	1 0
S.	33	17 0	37	11 2	41	9 0	33	17 0	36	14 0	47	2 1
N.	27	13 10	33	12 5	43	5 2	34	14 2	44	2 4	38	3 9
D. I.	30	16 4	36	10 4	47	2 1	39	9 2	39	10 1	49	1 0
D. II.	43	7 0	34	14 2	46	4 0	36	13 1	42	8 0	50	0 0
F.	33	16 1	41	6 3	46	3 1	36	10 4	45	2 3	44	5 1
C.	38	11 1	42	6 2	45	4 1	35	13 2	41	6 3	42	7 1

As we have seen, the figures given in the table do not directly measure the fineness of discrimination. In the manner described in the Introduction we have calculated the probable errors of the several observers for the four amounts of difference and give the results in Table XXII.¹

TABLE XXII.
PROBABLE ERROR WITH LIFTED WEIGHTS.

100:	104.	108.	112.	116.	AV.	V.
R.	7.4	9.8	7.3	8.3	8.2	.8
K.	6.4	8.1	7.2	8.2	7.5	.7
G.	6.5	5.1	6.8	7.	6.3	.6
L.	5.1	8.	4.8	6.6	6.1	1.2
S.	6.8	8.4	6.3	7.7	7.3	.7
N.	6.1	5.5	7.3	5.9	6.2	.5
D. I.	4.5	7.1	4.3	4.6	5.1	1.0
D. II.	3.2	7.1	4.3	5.6	5.	1.3
F.	4.7	4.3	6.	6.2	5.3	.8
C.	4.1	5.1	6.8	4.6	5.1	.8
Av.	5.5	6.8	6.1	6.5	6.2	.8
V.	1.2	1.5	1.	1.1	.9	

This table shows clearly the probable error of the several observers—that is, the difference in grammes which each of them could expect to distinguish 75 times out of 100. On the average, weights can thus be discriminated when they are

¹ In this calculation the doubtful cases have been divided equally between "right and wrong." Much discussion has arisen concerning the disposition of doubtful cases, but we think the correct solution has not been suggested. We believe it is found in the proper application of the probability integral. However, when there are but few doubtful cases, this would not give a result appreciably different from an equal division. As has been said, we think it best to let the observer divide his doubtful cases by guessing, and this has been done in our later experiments. When the observer is always right in his judgment his fineness of discrimination has not been measured. We must assume that errors would occur if the series were continued long enough, and have somewhat arbitrarily set 99 as the most probable percentage of right cases.

about as 100:106 (or 110:116) gm.¹ or when the difference is (say) $\frac{6}{108} = \frac{1}{18}$ of the stimulus. If we wish to know what difference can be distinguished 99 times out of 100 the figures in the table are to be multiplied by 3.45, or about $3\frac{1}{2}$. Thus, on the average, the difference between 100 and 121 gm. can be distinguished 99 times out of 100.

The table shows that the fineness of discrimination of the observers did not differ greatly, the probable error varying from 8.2 to 5 gm., or from about $\frac{1}{18}$ to $\frac{1}{22}$ of the stimulus. If the observers be classified we find that the average probable error for two women was 7.8, for two advanced students of biology 6.2, for three advanced students of psychology 6.2, and for the two writers 5.2. As 400 experiments were made with each observer the results must be considered accurate, but it should be remembered that in all anthropometric experiments the size of the error may be due to accommodation to the conditions of experiment, as well as to real differences in the senses or faculties. The smaller probable error of the writers (nearly the same for the two) is thus probably not so much due to finer senses as to more practice in careful observation.

From this point of view as well as for other reasons it is interesting to note the improvement with practice. We have therefore divided the experiments into five sets, each containing eighty judgments, and give in the accompanying table the percentages of right cases for the observers with increasing practice.

¹ It will be noticed that in these calculations the error of observation is assumed to be the same for the several weights. This assumption is not strictly correct, but cannot be avoided until the relation between the error of observation and the magnitude of the stimulus is known.

TABLE XXIII.

RIGHT CASES IN PERCENTAGES WITH INCREASING PRACTICE.

	I.	II.	III.	IV.	V.
R.	78	75	70	81	85
K.	78	79	74	80	74
G.	82	82	83	68	78
L.	70	89	85	90	83
S.	83	78	80	78	83
N.	84	75	84	84	83
D. I.	84	88	81	88	92
D. II.	86	90	83	88	89
F.	86	85	89	81	93
C.	90	87	86	86	88
Av.	82	83	81	82	85

The averages in this table show a slight improvement in accuracy of discrimination as the series were continued, but it is so little that it might be due to chance variations. Two sets were made on *D.*, and the second was slightly the better. The different series on the same observer vary considerably, and eighty judgments by the method of right and wrong cases would, therefore, scarcely be sufficient as a test. Ten experiments by this method would be worthless.

SEC. 43. *The Amount of Difference and the Degree of Confidence.*—In these experiments we used four amounts of difference, our object being to combine the method of the just noticeable difference with the method of right and wrong cases, and to study the application of the probability integral. As regards this latter, our experiments agree very well with the mathematical theory. The probable error is nearly the same, whether it be calculated from a small difference and a relatively small percentage of right cases, or from a larger difference and a larger percentage of right cases. With the smallest difference the probable error is the least, but this is most likely due to chance

variations. If the probable error became gradually smaller as the differences were made smaller, it would indicate either a larger probable error with the greater weights (which is doubtless the case but to a small degree) or, if considerable, the presence of factors other than the amount of difference, such as unconscious recognition of the weights by touch, or an inference from the order of presentation. Below (in Sec. 49) we give a curve showing the percentages of right cases for the several differences required by the probability integral, and those obtained by actual experiment.

By varying the amount of difference and making it in some cases so large that it was usually perceived correctly, we have combined the method of right and wrong cases with the method of the just noticeable difference. This shows clearly that the method of the just noticeable difference in its scientific form is simply a case of the method of right and wrong cases. We also see that it is not the most favorable amount of difference to choose for experiment; if the observer be always right, we have no satisfactory basis for comparison; and, further, a chance error, due, say, to complete distraction of the attention, affects the probable error more when the percentage of right cases is above 90 than when it is between 80 and 90.

The method of the just noticeable difference, in its less scientific form, has also been combined with the method of right and wrong cases by recording the degree of confidence of the observer. If the just noticeable difference be the difference the observer is sure that he can notice, it is that difference with which a was given; if it be the difference he feels some confidence that he can notice, it was that with which b was given. The number (which is also the percentage) of times a and b were given by the several observers is shown in the accompanying table.¹

¹ c was given in the remaining cases of the 100, unless the observer were doubtful. The number of times this occurred is shown in Table XXI.

TABLE XXIV.

THE DEGREE OF CONFIDENCE OF THE OBSERVERS WITH VARYING AMOUNTS OF DIFFERENCE.

100:	104.		108.		112.		116.		AV.	
	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.
R. . . .	10	45	19	46	25	46	33	46	22	46
K. . . .	41	0	51	0	62	1	64	0	54	0
G. . . .	47	37	64	22	71	15	72	21	63	24
L. . . .	13	72	22	69	32	58	39	53	26	63
S. . . .	60	36	51	45	83	15	81	16	69	28
N. . . .	34	29	40	28	47	21	62	26	46	26
D. I. . .	14	61	11	68	28	64	39	56	23	62
D. II. . .	5	50	8	54	24	59	30	57	17	55
F. . . .	3	31	17	29	23	34	37	34	20	32
C. . . .	1	16	2	28	9	31	12	58	6	33
Av. . . .	23	38	28	39	40	34	47	37	35	37

We see from the figures in the table that the number of times the observer felt confidence in the correctness of his decision usually increased as the amount of difference was taken greater.

Compared with the percentages of right cases, the fictitious averages are :

100:	104.	108.	112.	116.
% of times right,	69	77	89	94
% of times <i>a</i> and <i>b</i> ,	61	67	74	84

The confidence thus varies nearly as the percentage of right cases, and some reliance may, therefore, be placed on such introspection. We see, however, from the table, that different individuals place very different meanings on the degree of confidence. Some observers are nearly always quite or fairly confident, while others are seldom quite confident. If this table be compared with the preceding, it will, indeed, appear that those observers who felt the greatest degree of confidence in their judgment had the largest probable error, while those who were

most seldom quite confident had the smallest probable error. It is, therefore, absurd to measure the fineness of discrimination of an observer by the difference which he thinks he can notice, as is done when the method of the just noticeable difference is used in its usual form.

We give in Table XXV. the percentage of times the observer was right in his judgment when he used a , b and c respectively.

TABLE XXV.

PERCENTAGE OF TIMES THE OBSERVERS WERE RIGHT WITH EACH DEGREE OF CONFIDENCE.

	R.	K.	G.	L.	S.	N.	D. I.	D. II.	F.	C.	AV.
a	95	89	90	97	86	91	96	100	100	100	94
b	79	..	58	81	66	78	89	91	97	98	82
c	65	63	68	65	75	72	68	74	77	81	71

We see from the table that an observer is more apt to be right than wrong, even when he feels very little confidence in the correctness of his decision. We also obtain a rough measure of what reliance may be placed on the judgment of the observer.

SEC. 44. *The Constant Error.*—Table XXI. shows that most of the observers were the more often right in their judgment when the second weight was the heavier. The second weight seemed relatively heavier than the first, which increased the apparent difference when it was in fact the heavier, and decreased it when it was in fact the lighter. This constant error becomes eliminated in calculating the probable error, for we have in the one series $\frac{\Delta + C}{P. E.}$ and in the other $\frac{\Delta - C}{P. E.}$ so that C disappears in the average value of $\frac{\Delta}{P. E.}$ from which the probable error is calculated. If, however, we subtract the one equation from the other, we can find the relation of C to $P. E.$, and,

consequently, the value of the constant error. These values for the several observers and differences are given in the accompanying table.¹

TABLE XXVI.

CONSTANT ERROR IN DISTINGUISHING WEIGHTS WEIGHING ABOUT 100 GM.—METHOD OF RIGHT AND WRONG CASES—9 OBSERVERS—4000 EXPERIMENTS.

100:	104.	108.	112.	116.	AV. $\pm$
R.	-2.4	.4	-2.	0.	1.2
K.	4.	5.	3.	8.6	5.1
G.	6.5	5.3	7.	8.6	6.8
L.	.9	1.1	2.8	3.8	2.1
S.	.1	— .8	3.4	0.	1.3
N.	.7	3.5	— .8	4.3	2.3
D. I.	1.6	.5	1.3	0.	.8
D. II.	-1.1	2.5	3.	3.2	2.4
F.	.9	1.4	-1.1	1.5	1.2
C.	— .5	— .2	-1.5	0.	.5
Av. $\pm$	1.9	2.1	2.6	3.	2.4

The table shows that the constant error varied more than the probable or variable error. For two of the observers the constant error is about as large as their probable error. The constant error for *F.* and *C.* is small (within the limits of chance variations), and this may possibly be owing to the observers having unconsciously made allowance for a constant error, as they knew the tendency of other observers to overestimate the second weight. The constant error of the average increases regularly as the difference is taken greater. As is usually the case with constant errors, the overestimation of the second weight in these experiments is difficult to explain. At first sight it might be attributed to fatigue; but as many pairs of trials were

Owing to the limited number of experiments, some part of the errors given in this table is due to the variable error.

made in succession, fatigue could scarcely tell in a single pair. The overestimation of the second weight may possibly be due to the fact that it was the more recently perceived. But the various effects of contrast have not yet been explained.

SEC. 45. *Conditions Affecting the Perception of Lifted Weights.*—In the experiments which we have been discussing the weights were lifted as described, and the movement was always made in the same manner. It would evidently be an advantage to alter the nature of the movement, and from the variation in the probable error attempt to learn the factors concerned in the perception of lifted weights. With this object in view we made the movements in various ways, and give the results in the accompanying table. Only 200 experiments of each sort were made, and the probable and constant errors can only be regarded as approximate. The weights used were 100 and 108 gm., the probable and constant errors being calculated as described. The nature of the movement is given in the table, and will be described more exactly below. The series were made in the order in which they are given in the table, but the groups collected under *Av. Av.* were made simultaneously, and half of the experiments on lifting the weights up and down with one and with two hands were made at the close.

TABLE XXVII.

PROBABLE AND CONSTANT ERRORS IN LIFTING WEIGHTS 100:108 GM.—J. AND C.—
3000 EXPERIMENTS—I. 1891—I. 1892.

NATURE OF THE MOVEMENT.	PROBABLE ERROR.				CONSTANT ERROR.			
	J.	C.	AV.	AV. AV.	J.	C.	AV.	AV. AV.
Right to left ca. 40 cm. . .	5.7	5.1	5.4	5.4	— .4	— .2	— .3	— .3
Up and down, right w't first	8.8	7.6	8.2		— .4	— 1.5	— .9	
Up and down, left w't first	6.3	4.8	5.5	6.9	+ 1.3	+ 3.0	+ 2.1	+ .6
Two hands, right first . .	7.3	9.8	8.5		— 3.7	— .4	— 2.0	
Two hands, left first . . .	11.1	6.4	8.7	8.6	+ 4.7	+ 3.1	+ 3.9	+ .9
Up only, 16 cm.	8.6	9.8	9.2		+ 2.8	— 1.3	+ .7	
Down only, 16 cm. . . .	5.1	13.1	9.1	9.1	— .7	+ 1.0	+ .1	+ .4
From wrist only	7.5	8.2	7.8		+ 2.2	+ 2.3	+ 2.2	
From forearm only	6.2	6.6	6.4		+ 2.	— 1.7	+ .1	
From shoulder only . . .	10.3	8.2	9.2	7.8	+ 1.8	— 6.2	— 2.2	+ .1
First 4 cm., second 16 cm.	4.8	6.4	5.6		— .3	+ .7	+ .2	
First 16 cm., second 4 cm.	7.8	5.4	6.6		+ 5.6	— 3.3	+ 1.1	
First ½ sec., second 2 sec.	5.6	5.3	5.4		— 1.7	— 3.0	— 2.3	
First 2 sec., second ½ sec.	5.4	4.6	5.0	5.7	— 1.3	.0	— .6	— .4
Pressing the weights . . .	10.9	10.3	10.6	10.6	— 1.4	— 1.8	— 1.6	— 1.6
Averages.	7.4	7.4	7.4	7.4	+ .6	— .6	0	0

The average probable error for *J.* and *C.* is exactly the same—7.4 gm., or about $\frac{1}{14}$ of the stimulus. Throughout our experiments the results for *F.*, *J.* and *C.* agree closely. The average constant error is small for both *J.* and *C.*—less than $\frac{1}{10}$ of the probable error. It is negative for *C.*—that is, the second weight seemed relatively slightly the lighter, as was the case before. It is positive for *J.*, as was the case with eight of the ten observers given in Table XXVI.

In considering the probable errors for the different kinds of movements, it must be remembered that they (being the result of only 100 experiments with each observer) would vary considerably even were the movement always the same. However,

some reliance may be placed on the figures given under *Av. Av.* The probable error was smaller, and was subject to smaller constant errors when the weights were lifted across 40 cm., than when they were lifted up 8 cm. and set down again. The time of the movement in each case was one second. As before noted, a movement of about 40 cm. seems the most natural. *J.* was not told how long to make the movement, but made it on the average 42.6 cm. When the weights were lifted up and down the height was determined by loose horizontal silk threads, which marked the distance without offering resistance. The distance the weights were lifted by *J.* in series two and three was noted without the observer's knowledge. When the judgment was right the heavier weight was lifted on the average 7.9, the lighter 7.8 cm.; when wrong the heavier weight was lifted 7.9, the lighter 7.9 cm. The distance which the weights were lifted was, consequently, not proportional to the weight, and the observer cannot judge of the difference by the distance (or rate) as maintained by Müller and Schumann (*op. cit.*). The probable error was increased by about $\frac{1}{4}$ when the weights were lifted with different hands. It was considerably increased when the weight was lifted up only (16 cm.) or down only. It became larger when the movement was made from the shoulder only, the wrist and forearm not being bent. It seems, therefore, that the perception depends on sensations accompanying the movements of the wrist and forearm, but when either of these is present the error is not appreciably greater than when the three joints are used. When the rate of movement was altered so that the one weight was lifted four times as rapidly as the other, either by being lifted higher in the same time or the same distance more quickly, the probable error was not increased. This result was unexpected, and proves conclusively that we do not judge of difference in weights by the rate at which they are lifted. When

the weights were pressed by the fingers with force sufficient to lift 2 kg., the probable error was considerably increased. The observer is, consequently, helped by the lightness of touch, which normally is made just sufficient to hold the weights. The sensation may be partly touch and partly a muscular sensation from the fingers. When two hands were used the weight lifted with the right hand seemed relatively the heavier. But the constant errors are throughout within the limits of probable chance variations.

In all these experiments the observer's confidence in his decision was recorded. The confidence seemed to correspond exactly with the apparent amount of difference. Thus the observer said *a* if the difference seemed quite evident, *b* if the difference was apparent but not so great that he felt sure of the correctness of his judgment, *c* if he noticed but little difference and felt doubtful as to his correctness, and *d* if he could discern no difference, and felt that his judgment was a mere guess. The percentages of the times the several degrees of confidence were given were as follows :

	<i>a.</i>	<i>b.</i>	<i>c.</i>	<i>d.</i>
<i>J.</i> . . .	10	25	46	19
<i>C.</i> . . .	0	13	60	27

a or *b* is the just noticeable difference, and *d* is the threshold of German psychologists. The just noticeable difference (and also the threshold) would make *J.* a much better observer than *C.* No weight, consequently, can be laid on such determinations.

The percentages of times the observer was right in his judgment were as follows :

	<i>a.</i>	<i>b.</i>	<i>c.</i>	<i>d.</i>
<i>J.</i> . . .	93	86	74	65
<i>C.</i> . . .	x	97	81	60

It is interesting to note that when the decision of the observer seemed a mere guess, he was considerably more likely to be right

than wrong. This bears witness to the part played by subconscious mental processes in our daily life. *C.*'s judgment was more accurate than *J.*'s, as he was right a larger percentage of times when he felt some confidence, and wrong a larger percentage of times when he felt none. *C.* could notice no difference when the apparent difference (assuming the theory of the distribution of errors to obtain in this case) was less than $\frac{1}{2}$ the probable error, and could notice the difference with some confidence when the apparent difference was more than double the probable error. *C.* should by theory have been right in about 57 per cent. of the cases when he gave *d*, and in 97 per cent. of the cases when he gave *b*. These are nearly the percentages of right cases actually obtained, whence it follows that an observer can judge apparent differences in sensation with considerable accuracy.

PART V.

ON LIGHTS.

SEC. 46. *Introductory*.—Numerous experiments have been made with a view to testing Weber's law for the sense of sight. We need not give an abstract of these, as our own experiments do not bear directly on the subject. The early experiments by Bouguer, Lambert, Steinthal, Masson, Fechner and Volkmann seemed to show that the just noticeable difference is a proportional part of the stimulus. The conclusion of later experiments,¹ however, is that Weber's law holds only approximately and for lights of medium intensity.

In these experiments the figures obtained as expressing the accuracy of discrimination proper to the sense of sight place it very high. Sight has, accordingly, been described as the most accurate of the senses. But it is evident that in the experiments with shadows and with disks, as they have been carried out, the conditions are very different from those of experiments

¹ Volkmann, *Physiol. Untersuch.*, etc., Leipzig, 1863.

Aubert, *op. cit.*

Helmholtz, *op. cit.*

Delbœuf, *op. cit.*

Camerer, *Mon. Bl. f. Augenheil.*, 1877.

Kraepelin, *Phil. Stud.*, II. 1885.

Neiglick, *Phil. Stud.*, IV. 1887.

Merkel, *op. cit.*

Koenig und Brodhun, *Sitz. d. Acad. zu Berlin*, 1888.

Müller-Lyer, *Archiv f. [Anat. u.] Physiol.*, Sup. Bd., 1889.

The two papers last mentioned are of special importance. The German physiological archives and proceedings are, unfortunately, not included in the libraries of the University of Pennsylvania or Columbia College, and were not accessible to us while this paper was being written. Since going to press we have examined some papers in the Astor Library, and added references, but it is too late to give abstracts.

with lifted weights. In comparing a shadow with its background, the observer receives both impressions simultaneously, and attention passes from one to the other and back again, thus receiving a complex series of impressions. So it is also with variegated disks. In lifting weights, on the contrary, the impressions are successive. This must also be the case in experimenting on hearing and other senses. In our own experiments with lights we have tried to make the conditions similar to those of our experiments on weights. Our results, in connection with which the size of the illuminated area on the retina must, of course, be taken into account, do not give pre-eminence in discriminative power to the sense of sight as compared with the muscular sense.

SEC. 47. *Apparatus and Methods.*—The apparatus used in these experiments was planned by the writers. It is represented in the accompanying cut.

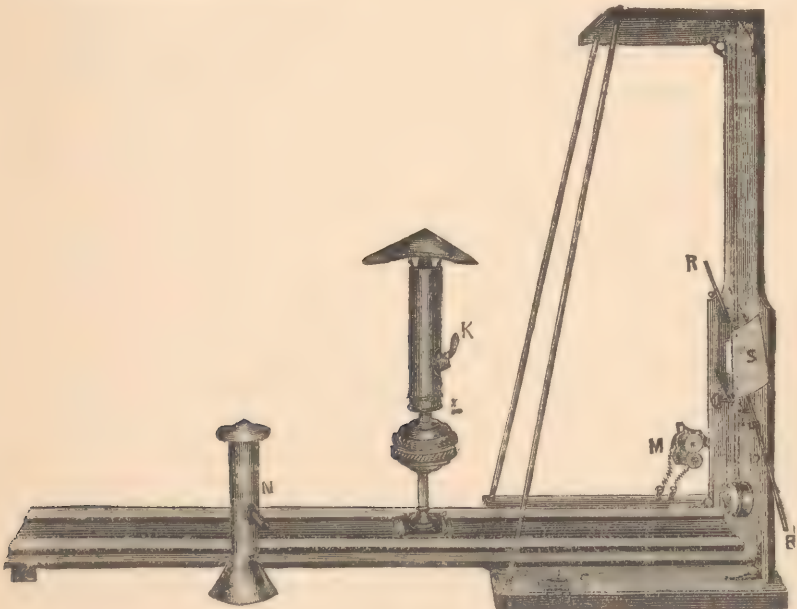


FIG. 67

A lamp (L), provided with a metal hood, runs on a wooden slide. At one end of the slide is an upright plank pierced by a hole. This aperture can be enlarged or diminished in size by pulling out or pushing in two brass rods (R R') which are attached to overlapping plates of metal so cut as to leave between their edges a square hole. A seconds pendulum (P) swings in front of this hole, and a sheet-iron screen (S), attached to the bar of the pendulum, covers the hole, when the pendulum is held up by the electro-magnet (M). There is in the hood of the lamp, opposite the flame, a small hole (15 mm.) with a hinged cap (K). As the pendulum swings freely, the light of the lamp shines for one second through the aperture in the upright plank, and then is for one second cut off by the screen on the pendulum. The pendulum, weighted by a heavy iron ball, can at any time be held up by the electro-magnet (M), the recorder manipulating the key (at C). The hooded lamp (N) gives the recorder light to keep his record and to measure the movement of the lamp.

In the following cut may be seen the positions of the observer, of the recorder, and of the several parts of the apparatus.

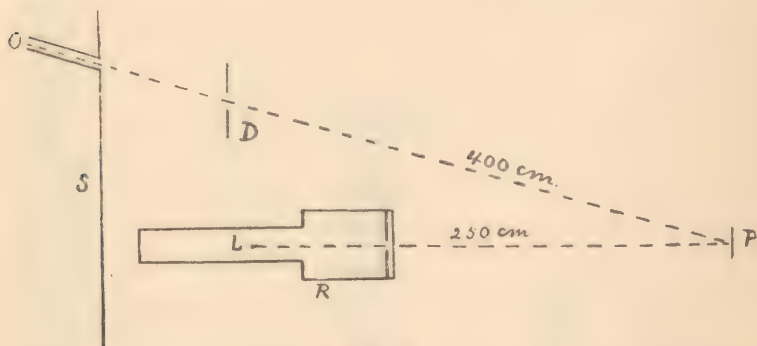


FIG. 68.

The observer (O), behind a wooden partition (S), looked with one eye through a tube fixed in the partition at a piece of white

paper (P) on the opposite wall and distant 4 M. A diaphragm (D) permitted him to see on this paper a circle 10 cm. in diameter. The lamp (L) illuminated this paper through the aperture in the upright plank, when the aperture was not covered by the screen on the pendulum. The lamp was 250 cm. distant from the paper when at its farthest point. The recorder (R) moved the lamp to or from the paper, and regulated the swinging of the pendulum by means of the key. During the experiments the room was darkened by the use of double blinds at the windows, and every effort was made to prevent scattered light escaping into the room from the two lamps.

The experiments were made as follows:—The observer having placed himself in position, the recorder gave him a signal to secure attention, and then, turning the key, allowed the pendulum to swing away from the magnet. The lamp¹ being, say, at what we may call the normal distance (250 cm. from the paper), the observer was thus given the standard sensation of light. With the return swing of the pendulum, and while the light was cut off by the screen, the lamp was moved nearer to the paper by a certain distance marked on the slide, so that the second light seen by the observer should be more intense than the first in the ratio of 100: 110, 120, 130 or 140. At its next return the pendulum was caught up by the magnet. Between the next two lights the movement might be from the 140 point to the 100 point, etc. Each light lasted one second, and there was an interval of one second between the two lights. The observer was required to state whether the second light in each pair seemed brighter or fainter than the first, and to express his degree of confidence by a , b or c . A record was made after each experiment.

¹ The lamp at the usual height was ten-candle power. To prevent the observer from gaining a familiarity with the absolute intensity of the normal light, the lamp was frequently turned up or down a little. These variations in intensity were not great.

Forty experiments were made in each series, in twenty of which the second light was fainter, and in twenty brighter than the first. Five experiments of each of these kinds were made in the one series for each of the four differences chosen. The results have been computed and tabulated as in the experiments on lifted weights. Of course the different experiments in each series were made in an irregular order, and the observer had no clue to guide him except the objective difference in the lights. Only the writers knew the results of previous experiments.

SEC. 48. *Results of Experiments.*—The results of 4000 experiments made on nine observers are contained in the accompanying table. The number of right, wrong and doubtful cases is given for each amount of difference and according to whether the second light was the fainter or the brighter.

TABLE XXVIII.
THE DISCRIMINATION OF LIGHTS—METHOD OF RIGHT AND WRONG CASES—9 OBSERVERS—4000 EXPERIMENTS.

SECOND LIGHT THE FAINTER.				SECOND LIGHT THE BRIGHTER.				
100.	110.	120.	130.	140.	110.	120.	130.	140.
	R. W. D.	R. W. D.	R. W. D.	R. W. D.	R. W. D.	R. W. D.	R. W. D.	R. W. D.
R.	37 13 0	42 8 0	49 1 0	50 0 0	28 22 0	29 21 0	31 19 0	37 13 0
K.	38 12 0	39 11 0	44 6 0	46 4 0	36 14 0	38 12 0	41 9 0	46 4 0
G.	37 9 4	46 2 2	48 2 0	50 0 0	18 27 5	31 18 1	40 7 3	48 2 2
L.	44 6 0	46 4 0	48 2 0	50 0 0	28 22 0	39 11 0	48 2 0	50 0 0
S.	48 2 0	50 0 0	49 1 0	50 0 0	14 35 1	29 20 1	33 16 1	46 4 0
N.	37 8 5	42 1 7	47 1 2	49 0 1	29 12 9	41 2 7	39 1 10	48 0 2
D. I.	42 2 6	45 1 4	48 1 1	50 0 0	21 19 10	32 9 9	36 11 3	40 7 3
D. II.	45 5 0	50 0 0	50 0 0	50 0 0	25 15 10	42 3 5	45 4 1	49 1 0
F.	43 6 1	48 1 1	47 0 3	50 0 0	22 18 10	33 8 9	42 2 6	49 0 1
C.	37 10 3	43 4 3	48 2 0	50 0 0	24 12 14	35 7 8	43 0 7	45 0 5

As the figures given in the table do not directly measure the fineness of discrimination of the observer, we have calculated, as in the experiments with lifted weights, the probable errors of the several observers for the four amounts of difference. The results are contained in Table XXIX.¹

TABLE XXIX.
PROBABLE ERROR WITH LIGHTS.

100 :	110.	120.	130.	140.	AV.	V.
R.	17.0	22.5	17.2	18.0	18.7	1.9
K.	10.4	18.3	19.3	19.2	16.8	3.2
G.	24.6	14.3	15.0	16.6	17.6	3.5
L.	10.0	12.4	11.5	11.6	11.4	0.7
S.	11.2	10.6	16.2	14.5	13.1	2.2
N.	10.8	14.5	13.8	11.6	12.7	1.5
D. I.	10.1	12.5	15.8	16.4	13.7	2.4
D. II. . . .	8.8	7.6	11.0	12.3	9.9	1.7
F.	11.0	10.6	12.8	11.6	11.5	0.7
C.	12.9	13.5	13.7	13.6	13.4	0.3
Av.	12.7	13.7	14.6	14.5	13.9	1.8
V.	3.3	3.0	2.1	2.4	2.3	.8

We have here the difference, given in hundredths, of the intensity of the light chosen as a standard, which each observer may expect to distinguish 75 times out of 100. On the average, lights can thus be distinguished when they are about as 100:114, or when the difference is about one-seventh of the stimulus. Reckoning upon this basis, we find that a difference, to be distinguished 99 times out of 100, would have to be about 48, or nearly one-half the stimulus. It will be noticed that the difference is here much greater than that obtained in the experiments with lifted weights, and our results would seem to contradict the

¹ As before, the doubtful cases have been divided equally between the right and wrong cases.

commonly accepted doctrine of the greater fineness of discrimination of the sense of sight. It should, however, be borne in mind that in these experiments the retina was illuminated over a comparatively small area, and but one eye was used. Had the area illuminated been greater, and had the light been seen with both eyes, a lesser difference could probably have been perceived. The lights used, moreover, were comparatively faint. We have as yet made no experiments to determine the influence upon our results of such changes in the conditions of experiment. In one perhaps very important respect, the experiments on lights and those on lifted weights were alike. The two lights were seen, as the two weights were lifted, successively, and the time allowed each stimulus, as well as the time between each pair of stimuli, was accurately determined and kept constant. Any experiments designed to determine the relative degree of fineness of discrimination proper to sight and the other senses must observe such conditions if trustworthy results are to be obtained.

The variation of the averages for the several observers from their average (2.3) shows that the difference in discriminative power was not great, though there is a greater disparity between the best and the worst series (9.9 and 18.7) than in the case of lifted weights. Classifying the observers as before, we find the average probable error for two women was 17.7; for two advanced students of biology, 14.5; for three advanced students of psychology (the first series on *D.* only being used), 13.2; and for the two writers, 12.4.

To show the effects of practice on accuracy of discrimination, the experiments have been divided into five sets, each containing eighty judgments, and we give in the accompanying table the percentages of right cases for each set in the case of each observer. The second series of experiments made on *D.* was, of course, made after the first series was completed. In this

table, however, it is treated like the other series. It will be noticed that it is considerably better than the first series on *D*.

TABLE XXX.
RIGHT CASES IN PERCENTAGES WITH INCREASING PRACTICE.

	I.	II.	III.	IV.	V.
R.	82	74	72	82	69
K.	82	81	74	91	81
G.	59	81	80	85	82
L.	86	87	84	86	90
S.	77	82	79	81	79
N.	84	85	80	86	80
D. I.	72	79	74	77	85
D. II.	87	85	87	91	92
F.	72	84	69	91	97
C.	81	76	74	87	87
Av.	78	81	77	86	84

The table shows a slight improvement in accuracy of discrimination in the successive sets of eighty judgments, but it is very slight, and much stress cannot be put upon it. It might be due to accidental variations.

SEC. 49. *The Amount of Difference and the Degree of Confidence.*—In these experiments, as in those with lifted weights, four amounts of difference were used. The probable error is, as the table shows, nearly the same whether calculated from the number of right cases with a lesser difference or the number of right cases with a greater. This verifies experimentally the mathematical theory of the relation of the number of right cases to the difference used. The probable error is, however, as before, somewhat smaller with the smaller differences, and this indicates the presence of some small disturbing factor. We give curves showing the results of our experiments on weights and lights, as compared with curves showing the relations re-

quired by the probability integral. The agreement is close, and the probable error, whatever the difference and corresponding

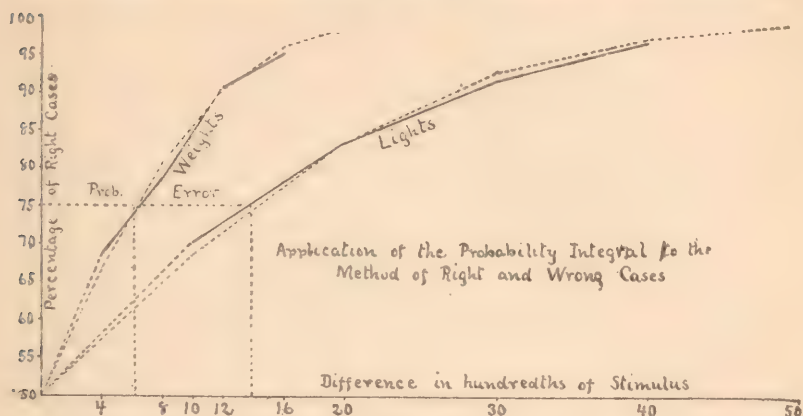


FIG. 69.

percentage of right cases, may, consequently, be used as a measure of accuracy of discrimination. The curves show clearly that under the conditions employed by us weights can be distinguished much better than lights.

By making the difference in some cases so large that the observer was nearly always right (100:140), we have combined with the method of right and wrong cases that of the just noticeable difference. It is scarcely necessary to point out again that this last-mentioned method, when used in any satisfactory way, is simply a form of the method of right and wrong cases, and that the difference employed is not the best difference to use in experiments by the method of right and wrong cases.

The observer was required in these experiments to use *a*, *b* and *c*, in the manner already described, to indicate the degree of his confidence in the judgment made. The accompanying table shows how many times *a* and *b* were given by the several observers.¹

¹ The rest of the time the observer gave *c* or *d*. The *d*'s are recorded in Table XXVIII. The figures in Table XXXI. are also percentages.

TABLE XXXI.

THE DEGREE OF CONFIDENCE OF THE OBSERVERS WITH VARYING AMOUNTS OF DIFFERENCE. IN %.

100.	110.	120.	130.	140.	AV.
	<i>a.</i> <i>b.</i>	<i>a.</i> <i>b.</i>	<i>a.</i> <i>b.</i>	<i>a.</i> <i>b.</i>	<i>a.</i> <i>b.</i>
R.	0 38	5 42	15 48	14 58	8 46
K.	1 6	2 6	8 13	12 18	6 11
G.	50 24	63 28	71 15	87 8	68 19
L.	6 44	16 52	35 55	70 26	32 44
S.	68 28	65 30	77 17	79 19	72 23
N.	16 23	28 34	54 26	69 20	42 26
D. I. . . .	11 33	20 34	33 48	56 34	30 37
D. II. . . .	3 26	1 65	15 65	47 48	16 51
F.	5 17	9 36	20 47	50 35	21 34
C.	1 13	4 23	17 32	28 35	12 26
Av.	16 25	21 35	34 37	51 30	31 32

It will be seen that the *a*'s and *b*'s increase as the difference increases, and as the percentage of correct judgments increases. Compared with the percentage of right cases the averages are :

	100.	110.	120.	130.	140.
Percentage of times right,	69	83	89	95	
“ “ <i>a</i> and <i>b</i> ,	40	56	71	81	

The confidence of the observer is, hence, a fair measure of the correctness of his judgment, but it is evident that *a* and *b* have a widely different meaning in the case of the several observers. *G.* and *S.* used *a* much more freely than did the others, and yet their probable error is not correspondingly small. That they used these letters in a looser sense than did the others is clear from the following table, which gives the percentage of times the several observers were right with each degree of confidence. *S.* was willing to say *b* when he was right only 62 per cent. of the time. He seems, indeed, to have made no real distinction between *b* and *c*. It is worth noting

TABLE XXXII.

PERCENTAGE OF TIMES THE SEVERAL OBSERVERS WERE RIGHT WITH EACH DEGREE OF CONFIDENCE.

	R.	K.	G.	L.	S.	N.	D. I.	D. II.	F.	C.	AV.
a. . . .	94.1	100	80.8	99.2	87.2	100	95.8	100	100	100	95.7
b. . . .	81.2	95.0	70.7	88.7	61.7	96.1	86.3	95.7	94.1	95.1	86.5
c. . . .	66.7	79.0	48.6	72.9	64.3	75.9	74.5	79.8	82.6	85.5	73.0

that when the discrimination was equally good the confidence was less with lights than with weights.

SEC. 50. *The Constant Error.*—All the observers showed a tendency to underestimate the second light, though the tendency was much more marked with some than with others. The amount of this error for each observer and for each degree of difference is shown in Table XXXII.¹

TABLE XXXIII.

CONSTANT ERROR IN DISTINGUISHING LIGHTS—METHOD OF RIGHT AND WRONG CASES—9 OBSERVERS—4000 EXPERIMENTS.

100:	110.	120.	130.	140.	AV.
R.	6.2	13.2	22.2	22.7	16.1
K.	0.9	0.9	3.7	0.0	1.4
G.	18.1	12.9	8.8	17.4	14.3
L.	7.7	5.8	0.0	0.0	3.4
S.	19.2	16.4	19.4	9.9	16.2
N.	2.9	0.9	5.9	0.2	2.5
D. I.	9.2	8.7	14.2	16.7	12.2
D. II. . . .	6.7	6.2	8.1	2.5	5.9
F.	8.4	9.4	5.7	0.0	5.9
C.	4.1	4.5	2.6	6.9	4.5
Av.	8.3	7.9	9.1	7.6	8.2
V.	4.3	4.2	5.7	7.2	5.2

¹ The constant error was obtained here as in the experiments on lifted weights.

It will be observed that the error varies considerably with different observers, and, indeed, with the same observer as the difference is greater or less. The averages do not increase with the difference, as is the case in the experiments with lifted weights. It is interesting to note that in no case was the first light underestimated, and in this respect the error is more constant than it is in the experiments with weights.

As we have seen, when two weights are successively lifted, there is a sufficiently marked tendency to overestimate the second. When two lights are successively looked at, there is a still more marked tendency to underestimate the second. The cause of this underestimation is not clear. Fatigue would not seem to account for it in an entirely satisfactory manner, for the second light in each pair was the one which seemed fainter, and the successive pairs of lights did not grow proportionally fainter as the series was continued. To test this more fully we made two series of ten experiments each upon *C.* and two upon *F.* The pendulum was allowed to swing freely eleven times, while the observer compared the eleven resulting sensations, and described them to the recorder. The intensity of the objective stimulus was kept the same. The results of these experiments were as follows:

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
C. I,	<i>f</i>	e	f	f	f	e	f	e	f	f
C. II,	<i>e</i>	f	f	e	f	e	<i>f</i>	e	f	f
F. I,	<i>f</i>	f	e	e	e	e	e	f	f	e
F. II,	<i>f</i>	e	f	e	f	f	e	e	e	f

Here *f* indicates that the light it represents appeared fainter than the one preceding, *e* that the light it stands for appeared the same as the one preceding. The letters in italics indicate that the difference was distinct. The writers have little confidence in the judgments not marked by letters in italics. The judgments had to be made rapidly, and the differences were felt

to be slight. They are, however, confident that they were never inclined to regard a light as brighter than the one before it.

SEC. 51. *Memory for Weights and Lights*.—In making experiments on the perception of small differences, the time elapsing between the sensations to be compared should not be neglected. In the experiments hitherto described the interval was always one second. It is possible that two seconds or longer might be a more favorable interval for comparison, but were the interval further lengthened the first sensation might be expected to fade from memory, while the rate of forgetting would be measured in terms of the error of observation. The study of memory for sensation is thus an attractive subject for psychological research, one quantity being studied as the function of another. Indeed, the earliest workers¹ in the field of psychophysics made a few experiments on the subject, but they did not continue them long enough to obtain definite results. Wolfe² carried out extended experiments on memory for the pitch of tones. He found about two seconds the most favorable interval; after this the percentage of right cases decreased, at first rapidly and then more slowly, until with an interval of sixty seconds the probable error is about five times as great as with one second. Lehmann³ made a few experiments with shades of gray produced by revolving wheels, and obtained results agreeing with Wolfe's. On the other hand, Paneth⁴ recently found that intervals of time could be reproduced nearly as well after longer (up to five minutes) as after shorter intervals; and Exner, who communicates these results, says that another experimenter obtained similar results with lights. The important monograph on

¹ Weber, Hagelmayer and Fechner.

² H. K. Wolfe, *Philos. Stud.*, III. 1886. Wolfe finds that the ratio of right to wrong cases is inversely proportional to the logarithm of the time. But accuracy of discrimination is not directly measured by the percentage of right cases.

³ Alfred Lehmann, *Philos. Stud.*, V. 1888.

⁴ J. Paneth, *Centr. f. Physiol.*, IV. 1890.

Memory by Ebbinghaus,¹ in which more complex impressions were used, is well known.

Our own experiments on memory were the first we made. They were postponed in order that we might study the method, and were not afterward completed. We used seven intervals varying from one second to about one minute, the experiments otherwise being conducted in a manner similar to those already described. The time of stimulation was one second. The normal weight was about 100 gm., and the difference usually 8 gm. The light was that described, the difference being one-fifth of the whole amount. The weights and lights were usually varied within narrow limits in order that the observer might not learn them, and thus be helped in his decision. On the weights 100 experiments were made with each interval, and on the lights 180, except with the 31 and 61 second intervals, with which only 80 were made. Ten different observers took part in the experiments. Owing to the large number of experiments required by the method of right and wrong cases, the probable errors given in the table are only approximate. The variable and constant errors have not been distinguished. In taking the average the probable errors with the lights have been halved in order to reduce them approximately to the standard of the weights.

TABLE XXXIV.

MEMORY FOR WEIGHTS AND LIGHTS—PROBABLE ERRORS FOR INTERVALS FROM 1 TO 61 SECS.—10 OBSERVERS AND 1760 EXPERIMENTS—V. TO XII. 1888—140 EXPERIMENTS—XII. 1891.

INTERVAL IN SEC.	1.	3.	5.	9.	15.	31.	61.	AV.
Weights 100 gm. P. E.	9.8	5.8	8.1	7.6	15.2	12.4	8.3	9.6
Lights 100 ½ P. E.	8	11.4	10.2	7.7	9.3	[11.8]	[10.2]	9.8
Average	8.9	8.6	9.1	7.6	12.2	12.1	9.2	9.7

¹ Herm. Ebbinghaus, Ueber das Gedächtniss, Leipzig, 1885.

The accompanying curve shows the relation between the probable error and the interval between the stimuli.

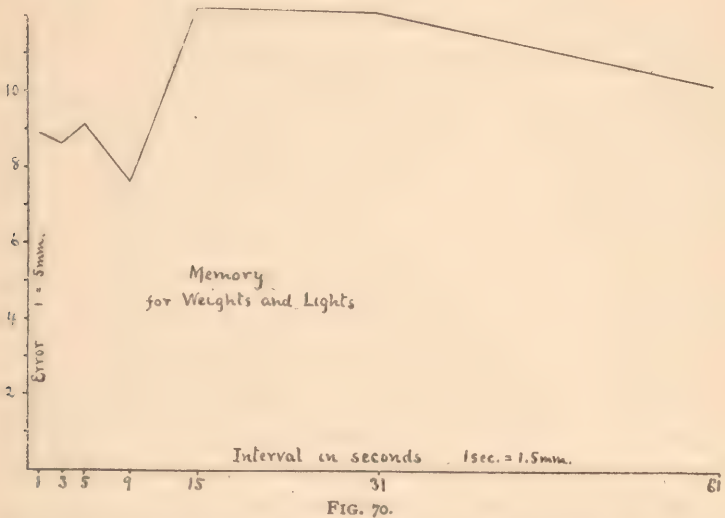


FIG. 70.

The experiments were not sufficient to determine exactly the probable errors with the different intervals, but these evidently do not become much larger as the interval is taken longer. The error of observation seems to be nearly the same so long as the interval is not over 9 sec., after which it is increased by about one-third. The smaller probable error with 61 sec. is most likely accidental. We had supposed that the increase in the error of observation with the larger interval would be more decided; the comparison becomes much more difficult to the observer, especially for the three longest intervals. The memory-image seems to last up to 9 sec., after which the observer does not so much compare the sensations as decide on the approximate intensity of each sensation separately, and compare the decisions. For example, the first sensation seems weaker than was expected, and this decision is remembered during the interval rather than the intensity itself.

CONCLUSION.

SEC. 52. *Psychophysical Methods*.—The method of the just noticeable difference—in which an observer finds a difference which he can just perceive—is not satisfactory. If the observer simply choose a difference which he thinks he can always or usually perceive, the result is without objective criterion. Indeed our experiments show that those who think they can perceive the smallest difference are apt to be the worst observers. If the percentage of mistakes made by the observer be recorded, this method becomes a case of the following. But “the just noticeable difference” is not a convenient difference to use in the method of right and wrong cases. If the percentage of right cases be very large, a single chance variation greatly affects the average. If there be no mistake we have indeed found a difference which can be perceived, but not the difference which can just be perceived, nor any other quantity which can be used as a measure of discrimination. If the just noticeable difference be interpreted by the observer as a difference apparently equal to some other difference, the method is reduced to that of estimated amount of difference.

The method of right and wrong cases—in which two stimuli nearly alike are presented to an observer, and he is required to say which seems the greater—is the most accurate of the methods. It requires a considerable number of experiments—at least 100—and the number must be the greater, the less practised the observer. The method is consequently not well suited for provisional, anthropometric or clinical purposes. The percentages of right cases obtained do not directly measure the fineness of discrimination. The probable error, that is the difference with which the observer should be right 75 per cent. of the time, is the most convenient measure of

discrimination. The probability integral may be used to calculate the probable error, when the amount of difference is known, and the percentage of right cases is greater or less than 75. It is better not to allow the observer to give doubtful as his decision, but the confidence felt by him in its correctness may be recorded with advantage. The observer is more apt to be right than wrong even when he feels little or no confidence in his decision. Some observers are not confident unless they are, in fact, right, while others are often confident when they are wrong. By recording the degree of confidence and using comparatively large differences the method of the just noticeable difference may be combined with that of right and wrong cases. The probable error and the constant error may be determined separately. The latter is more irregular than the former. It is worth noting that this most accurate method of observation has not been used in the physical sciences. Psychology has hitherto been much indebted to physics for its methods, but the obligation will soon be mutual.

The method of average error—in which an observer makes one stimulus as nearly as possible like another—is in many cases the most convenient of the methods. It is closely related to the preceding, as the probable error can be found either from the average error or from the percentage of right cases. The probable error of the just noticeable difference or of an estimated amount of difference may also be determined, and the several methods thus combined. The error obtained by the method of average error is complex, being partly an error of adjustment and partly an error of perception. These errors may be separately determined by requiring the observer to judge the stimuli by the method of right and wrong cases after they have been adjusted. The average error may be analyzed into a constant and a variable error. The distribution of the

errors tends to follow the probability curve. This method can be used to special advantage when only a few experiments are made, as a result is reached more quickly than by the method of right and wrong cases.

The method of estimated amount of differences—in which an observer judges the definite quantitative relations of stimuli, as in making one difference equal to or double another—gives variable results. The observer probably does not estimate quantitative relations in sensation, but quantitative differences in the stimuli learned by association. It is, consequently, an open question whether the differences in sensation are qualitative or quantitative.

Great care should be taken in psychological experiments to keep all the conditions constant except the variable to be investigated. The observer should not know the results of preceding experiments nor the objective relations of the stimuli. Experiments should not be rejected because they make the averages less accordant. The results of experiment depend on accommodation to the conditions of experiment as well as on differences in senses or faculties, and these factors should be separately studied.

SEC. 53. *The Error of Observation and the Magnitude of the Stimulus.*—Weber's law, according to which the least noticeable difference is proportional to the magnitude of the stimulus, does not hold for movement, as the least noticeable difference (or the error of observation) increases more slowly than the stimulus. Fechner's law, according to which the sensation increases as the logarithm of the stimulus, does not hold, as it rests on Weber's law and on assumptions which are probably incorrect. As there is no logarithmic relation between mental and physical processes, the psychophysical, physiological and psychological theories put forward to account for it are superfluous.

When amounts of difference in movements are estimated, the stimuli tend to be judged in their objective relations, and not as the logarithm of these. The results obtained by the method of right and wrong cases, and, by the method of average error, determine the error of observation. This is a physical quantity. Its correlation with other physical quantities (for example, the magnitude of the stimulus) depends on psychophysical conditions, and offers an important subject for psychological research. A mental quantity is not, however, directly measured. The error of observation usually increases as the stimulus is taken greater, but more slowly than in direct proportion to the magnitude. If the errors made in observing two stimuli of the same sort be combined, they will not be twice as large as the average error, but will equal the average error multiplied by the square root of two. This results both from theory and from our experiments; consequently, if two magnitudes, say two seconds, be observed continuously, the combined error in observing the two seconds would tend to equal the error in observing one second multiplied by square root of two, and generally the error in observing a magnitude, extensive or intensive, would increase as the square root of the magnitude. The summation of errors in this manner seems to account perfectly for the usual increase of the error of observation ("just noticeable difference") with larger magnitudes. The error would increase as the square root of the magnitude if each fraction of the magnitude, physically equal, were, in fact, subject to the same error of observation. In actual perception this would seldom or never be the case, but most of our experiments give an error of observation more nearly proportional to the square root of the magnitude than directly proportional to the magnitude. We, therefore, substitute for Weber's law the following : *The error of observation tends to increase as the square root of*

*the magnitude, the increase being subject to variations whose amount and cause must be determined for each special case.*¹

SEC. 54. *The Extent of Movement.*—Experiments on the extent of movement were made by moving the finger along a scale fastened to the edge of the table. The four distances chosen were 100, 300, 500 and 700 mm. Time was kept by a seconds pendulum, one second being allowed for each movement, and one second for the interval between the two movements to be compared. Experiments were made by the four psychophysical methods on one observer, and by the method of average error upon two others.

The attempts to mark off a distance just greater and one just less than 500 mm. resulted in, respectively, 539.4 and 477.2 mm. The distance marked off in separate experiments was highly variable. Even for groups of 100 experiments the average just noticeable difference varied, for the attempt at just greater, between 60.1 and 21.5 mm., and for the attempt at just less, between 37.7 and 4.8 mm. In striking contrast with these figures was the slight degree of variation in the variable error and its variation. For instance, where in two groups of 100 experiments each, the just noticeable difference was 60.1 and 21.5 mm., the corresponding variable error was 9.8 and 8.9. The highly variable character of the just noticeable difference makes it of small value in psychophysical experiment.

By the method of estimated amount of difference three kinds of experiments were made. It was attempted to halve 500 mm., to double 300 mm., and to find the mean between 300 and 700 mm. The results of these experiments were all contrary to Fechner's law, the attempt to halve 500 mm. resulting in a distance of 305.2, the attempt to double 300 giving one of 560.1,

¹ One of the writers (*G. S. F.*) does not give unqualified assent to the subject-matter of this section. See the note to Sec. 8.

and that to find the mean between 300 and 700 giving one of 512.4. In these experiments the variable error was, in relation to the whole extent of the movement made, greater than in the experiments by the method of just noticeable difference.

The experiments by the method of average error consisted in attempts to measure off on the scale 100, 300, 500 and 700 mm. The variable errors for these magnitudes (with three observers) were 10.0, 18.7, 21.1 and 17.2. The best of the observers was about three times as accurate as the worst. The variable error does not accord with Weber's law, but increases much more slowly than the stimulus. For all the observers it was actually smaller for 700 than for 500 mm., this probably because the distance was nearly the limit which could be reached, and the observer was helped by the strain.

In the experiments by the method of right and wrong cases the stimuli used were 500 and 510 mm. When the second stimulus was the greater, 75 per cent. of the judgments were right, and when the second was the less, 71.8 per cent. The probable error (the difference which could be distinguished 75 per cent. of the time) is 11 mm. The first movement was slightly underrated, the constant error being .7 mm. The degree of confidence expressed by the observer was a fair index of the objective correctness of his judgment.

SEC. 55. *The Force of Movement.*—A dynamometer may be used to advantage in studying the discrimination of the force of movements, but the clinical dynamometers are too inaccurate for scientific experiment. In making experiments on movement the observer can himself give the first or normal movement as well as the second or judgment movement, and the two movements will thus be made and perceived under like conditions.

“The just noticeable difference” in the force of movement varied greatly, not being proportional to the error of observation,

but more accordant results are obtained if the probable error be found by taking into account the number of mistakes made by the observer. The average error of the just noticeable difference may also be used as a measure of discrimination. The just noticeable differences (for two observers) for about 2, 4, 8 and 16 kg. were, respectively, about $\frac{1}{5}$, $\frac{1}{7}$, $\frac{1}{9}$ and $\frac{1}{18}$ of the stimulus. The variable errors of observation were, respectively, .12, .20, .37 and .41 kg., and the probable errors obtained by taking the percentage of the errors into account were, respectively, .14, .26, .40 and .45 kg.

Experiments by the method of average error gave (for five observers) variable errors .19, .29, .43 and .46 kg.; for the magnitudes 2, 4, 8 and 16 kg., respectively. The worst of the five observers had an error about one-third larger than the best. Some observers are relatively better with the weaker, some with the stronger movements. There were considerable constant errors varying with different observers. The second pull was too great with the smallest magnitude, and usually too small with the largest. Neither the just noticeable difference nor the error of observation is a proportional part of the stimulus. Weber's law, consequently, does not hold for the force of movement. The error of observation is nearly proportional to the square root of the magnitude.

The error, when two movements are made as nearly alike as possible, is partly an error of perception and partly an error of adjustment, and these two factors may be separated. The error of perception was, on the average, about twice as great as the error of adjustment, but the error of adjustment was relatively the smallest for the best observers. The errors in making two movements as nearly alike as possible tend to be distributed as required by the probability curve. The combined error obtained by adding algebraically the errors in pairs is nearly equal to the average error multiplied by the square root of two.

Experiments by the method of estimated amount of difference showed that the force of movements tends to be estimated in their objective relations, and not as the logarithm of these. The results are variable and subject to large constant errors.

SEC. 56. *The Time of Movement.*—Apparatus can be constructed which will measure accurately and conveniently the time either of a slow movement or of a quick blow. When movements are discriminated it is an advantage to let the observer adjust the time of the first as well as of the second movement.

The average errors in judging the time of movements (50 cm. in extent) with the arm, lasting about $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{2}$ and 1 sec., were (as the average of four observers) .014, .028, .052 and .100 sec., respectively. The error of the worst of the observers was about twice that with the best.

With $\frac{1}{2}$ and 1 sec., when the time of the two movements seemed equal, the second was the slower; when two blows in succession are made as quickly as possible, the second is the quicker and seems the quicker. $\frac{1}{2}$ sec. seemed less than half of 1 sec., and more than double $\frac{1}{4}$ sec.

The results obtained by analyzing the error into an error of perception and an error of adjustment, and from the distribution of errors and summation of errors, were nearly the same as with the force of movement.

The time of the quickest possible blow (50 cm. in extent) varied (with four observers) from .085 to .181 sec. While the rate of movement varies considerably with different observers, its average variation under like conditions is small—for a good observer .005 sec. The time was about the same for the right and left hand, and the rate was nearly uniform. The rate of movement should be used in the study of diseases of the nervous system.

Within the limits investigated the extent of movements can be judged better than the force, and the force better than the time.

SEC. 57. *Lifted Weights*.—The probable error in discriminating lifted weights weighing about 100 gm. varied (for nine observers) from 5 to 8.2 gm., the average being 6.2 gm. This is the difference which could be correctly distinguished three-fourths of the time. The difference which could be correctly given 99 times out of 100 would be about 21 gm. The probable error is nearly the same, whether calculated from a large difference and large percentage of right cases, or from a small difference and smaller percentage of right cases. The confidence felt by different observers in the correctness of their judgment varies greatly, and is not proportional to their fineness of discrimination. The constant error can be calculated. In these experiments it varied from .5 to 6.8 grammes. The second of the two weights seemed relatively the heavier to nearly all the observers. In judging the accuracy of discrimination of an observer, both variable and constant errors should be considered.

The probable error is not greatly altered when the manner of lifting the weights is altered. It becomes larger when the weights are lifted with different hands, or up or down only, or are pressed with the fingers. It is scarcely altered when one weight is lifted four times as high or four times as fast as the other.

SEC. 58. *Lights*.—In our experiments on lights, apparatus was devised to give the observer two sensations of light in succession, each lasting one second and one second apart. The conditions were thus similar to those in the experiments with lifted weights. The lights compared were as 100 to 110, 120, 130 and 140. The probable error (given in hundredths of the intensity of the stimulus) varied, for nine observers, from 9.9 to 18.7, with an average of 13.9. Reckoning upon this basis, a

difference, to be correctly given 99 times out of 100, would have to be about 48, or nearly half the stimulus. This large figure may be due partly to the fact that the illuminated area on the retina was small and the lights comparatively faint, but it was probably chiefly due to the sensations being successive. We consider it an advantage to have the sensations successive, as the conditions can thus be kept constant, and sight can be compared with the other senses, muscular sense, hearing, etc. Different observers differed much in their degree of confidence in the correctness of their judgment, and their degree of confidence was no indication of the relative fineness of their power of discrimination. For the same observer, however, the degree of confidence corresponded fairly well to the degree of objective accuracy. All the observers showed a tendency to underestimate the second light, the constant error varying from 1.4 to 16.2. Under the conditions employed the muscular sense is about again as accurate as the sense of sight.

Memory for sensations may be studied by increasing the interval between the two stimuli to be compared, the probable error of an observer measuring his rate of forgetting. Observers remembered lifted weights and lights so well up to 9 sec. that their error of observation was not increased. When the time was from 15 to 61 sec., the error was increased by about one-third. This is contrary to the common view, according to which we are supposed to forget most rapidly at first.

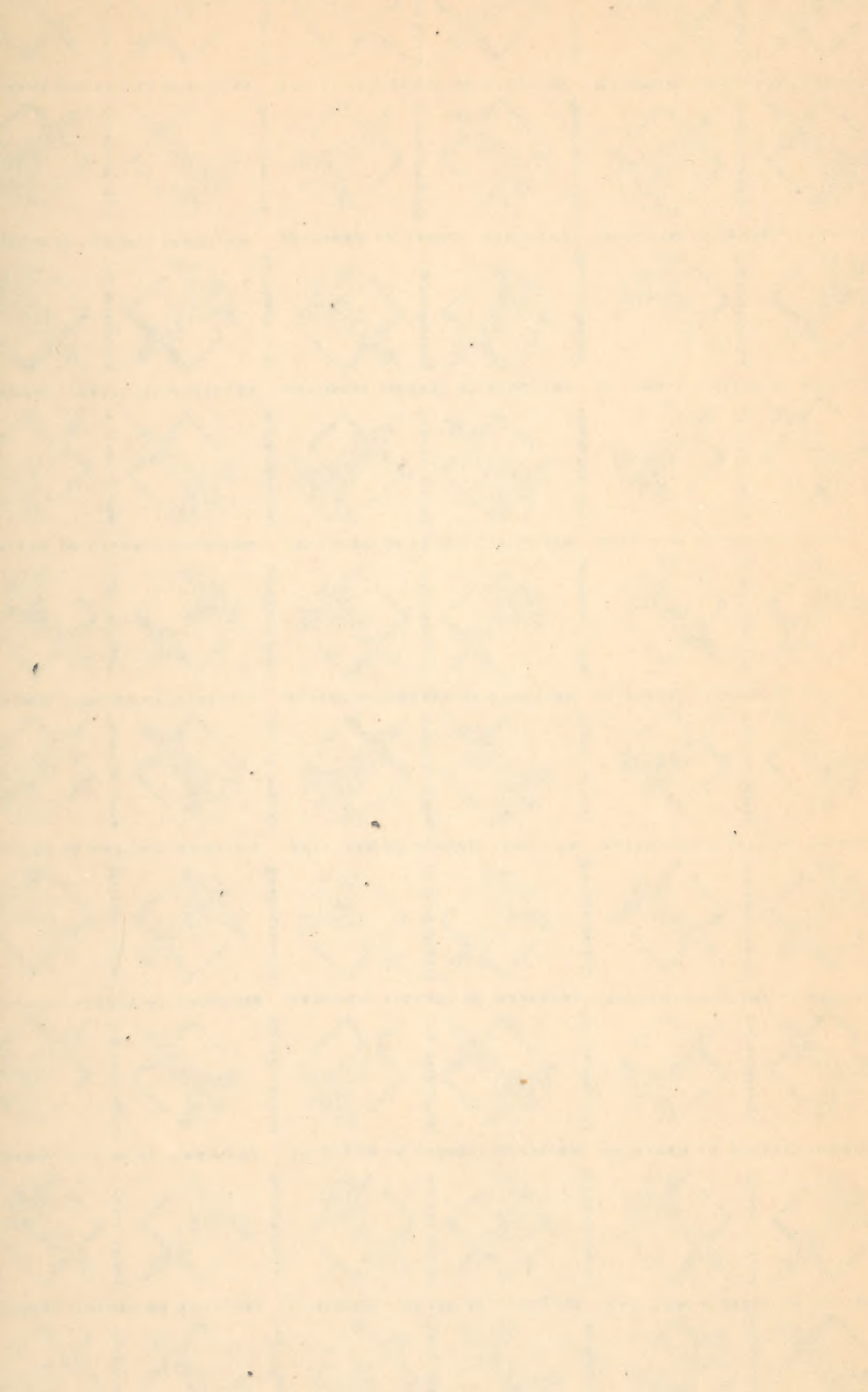
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